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BIOGRAPHICAL MEMOIRS
OF FELLOWS
OF THE ROYAL SOCIETY

VOLUME 2

BIOGRAPHICAL MEMOIRS OF FELLOWS OF THE ROYAL SOCIETY

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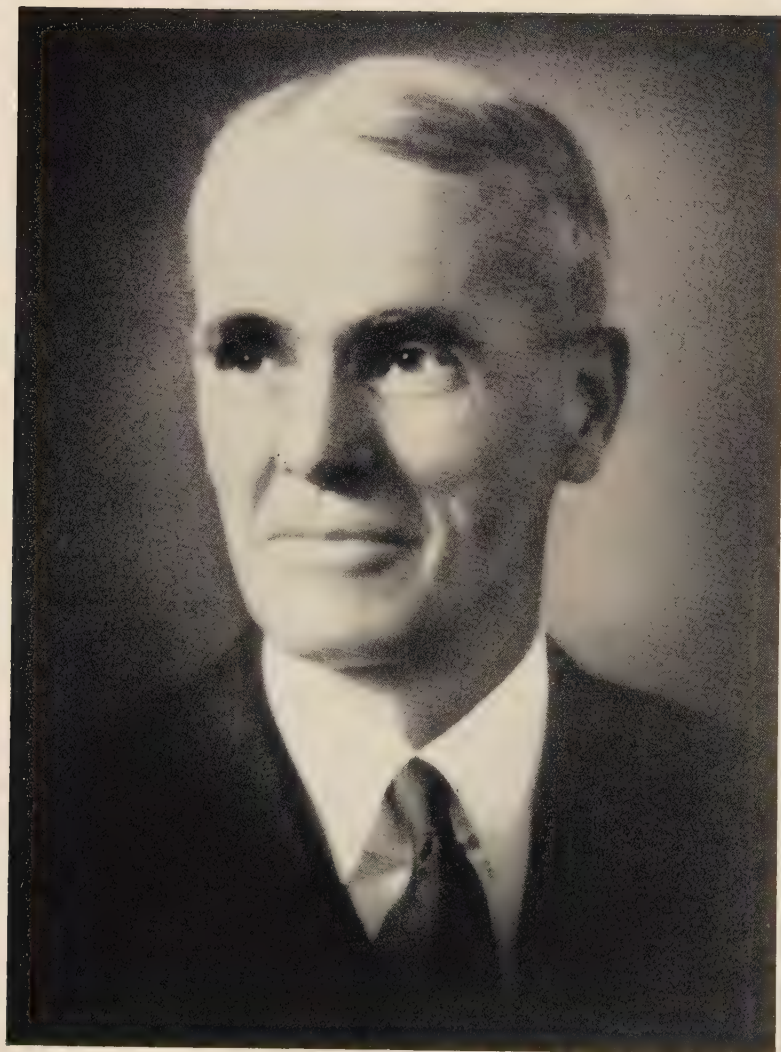
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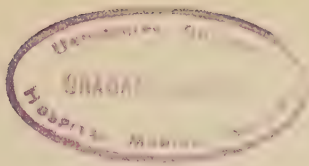
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Walter S. Adams



WALTER SYDNEY ADAMS

1876-1956

WALTER SYDNEY ADAMS was born on 20 December 1876, in the village of Kessab near Antioch in Northern Syria, then a province of the Turkish Empire. His father and mother, Lucien and Nancy Adams, were missionaries under the American Board of Commissioners for Foreign Missions with headquarters in Boston. They were both college graduates—Lucien of Dartmouth College and Andover Theological Seminary, Nancy of Mount Holyoke College. Walter was the youngest of the family. In the village in which he lived there were no schools except one for Mohammedan children and one for Armenians, so he received his earliest training in the elements of arithmetic, grammar and geography from his mother. For books he had his father's library which, apart from theological books, consisted largely of histories and classical text books and treatises. At the age of six he knew more of the history of Athens and Rome, and of the campaigns of Alexander the Great and Hannibal than of the rise and development of the United States. In 1885, when Adams was eight years old, he moved with the family to the United States and settled in the village of Derry, New Hampshire, his father's old home.

His education was continued at first at the village school and then at Pinkerton Academy, a privately endowed secondary school in the village. In 1890 his father returned to Syria and Adams was sent to St Johnsbury Academy in northern Vermont where he completed his college preparatory course. He then, largely for health reasons, spent a year on a Massachusetts farm and a year at Phillips Academy, Andover. Here his early classical education was followed by study in mathematics, physics and chemistry. He graduated from Andover in 1894 and then entered Dartmouth College where he studied under Frost; he graduated A.B. in 1898. This was followed by two years' graduate study at the University of Chicago where he worked under Moulton, Laves and Bolza for his A.M. degree. In 1899 he published his first paper on 'The polar compression of Jupiter'. At the suggestion of Hale and Frost he proceeded in 1900 to Munich to work for a Ph.D. degree under Seeliger and Schwarzschild; but at the end of a year he received from Hale the offer of an appointment as computer and general assistant at the Yerkes Observatory, which offer he accepted and he returned to live in Chicago.

At Yerkes he commenced his study of the radial velocity of stars to which he was to devote much effort in later life, working at first with Frost; he also served for a year as an instructor in astrophysics and as an assistant astronomer,

before he was invited by Hale in 1904 to join him, along with Ellerman and Ritchey, in the task of establishing a new observatory on Mt Wilson in California. Actually Adams went with Hale from Chicago to California before the scheme for the high tower solar telescope and the 60-inch stellar telescope had been approved by the founders of the Observatory, the Carnegie Institution of Washington, and before the necessary large financial grants had been decided upon. Not for the last time was Adams to witness the disappointments and frustration attending successive steps by Hale in pressing forward his schemes for larger instruments for the Mt Wilson Observatory and the ultimate success achieved on each occasion.

Adams accompanied Hale on his second visit to the top of Mt Wilson in 1904 and for the rest of his life he was to be the devoted colleague of Hale and servant of the Observatory. He was to be an Assistant Astronomer 1904-9, Assistant Director or Acting Director 1909-23 (during which period he was frequently actually serving as Director during the absence of Hale for illness or other reasons), and Director 1923-46. He and Hale worked closely together in the building and planning of the observatory. Conditions of life on the mountain were very primitive at first and difficulties of transportation up the mountainside were serious. A foot trail was improved into a mule trail fit for pack transport; this was in turn widened to allow for a mule waggon team and later for a truck. Adams had many amusing tales about the mules and their burdens, both living and material. The early widenings of the trail had to be done by hand; when some ten years later the track was widened again for the transportation of the parts of the 100-inch telescope power shovels and drills were available to help.

It was good that Adams was a fine walker and athletically active. In the early days observers would walk up the mountain—an eight mile trek—adjust or change instruments in the afternoon and observe all night. Adams walked up the mountain trail some 200 times, though, charged as he was with the erection of the buildings on the summit, he naturally stayed up for long spells in the early days. The single log cabin known as the Casino, which was the only available building at first, was replaced by the Monastery for the resident observers. The building for the Snow solar telescope had to be designed and erected and spectrographs installed: and as soon as regular observations had commenced the 60-ft. solar tower telescope had to be erected and preparations made for installing the 60-inch stellar telescope. The transportation, construction and erection of this telescope and its building provided a major problem for nearly two years.

In addition to the supervision of the building programme on Mt Wilson Adams was occupied in designing spectrographs for the 60-inch telescope for use at the primary focus, the Cassegrain focus and the coudé focus: these had to be designed to suit the proposed programme for the telescope; this included radial velocity investigations comparable with those carried out on other large telescopes but extended to fainter stars, and the application to much higher dispersion of the greater light-gathering power of the telescope.

Later a laboratory had to be installed for the use of Gale and King in their study of spectra under varying conditions of temperature and pressure. Optical tests also were carried out by Adams, for instance on Ritchey's 30-inch mirror for the Helwan Observatory, and later a comparison of the optical performance of the 60-inch and 100-inch mirrors.

With all this administrative work on his hands and while waiting for the erection of the 60-inch telescope, which was to be used to continue his studies of stellar radial velocities, Adams carried out with Hale investigations on the spectra of sunspots, paying especial attention to the lines widened or doubled in sunspots, and to those which were intensified or weakened. Hale's discovery of magnetic fields in sunspot dealt largely with the first group of lines. Work in the laboratory with Gale showed that the lines which varied between spot and disk also varied in arcs carrying currents of differing strength and also in different parts of the arc. Hale, Adams and Gale identified the source of these changes as differences in temperature. Alfred Fowler carrying out simultaneously in London work on condensed spark discharges regarded electrical excitation as the main factor concerned. As Fowler developed the modern theory of ionization, thus opening a new epoch in the rational interpretation of spectra, it became clear that the 'enhanced lines' which were weakened in spots were lines due to ionized atoms. Their greater strength in the chromosphere was not understood at first but was explained subsequently by Saha as a result of diminished pressure.

The work on the spectra of sunspots was carried out at first with the Snow telescope and spectrograph mounted horizontally; the erection of the 60-ft. tower telescope with its improved seeing and the higher dispersion given by the 30-ft. spectrograph greatly improved the quality of the spectrograms available. Adams with Hale and others was able to identify titanium oxide bands in spots, to examine the differences in intensities and wave-lengths of lines between the spectra of the centre of the solar disk and the limb, to study the flash spectrum without an eclipse and to verify the presence of convection currents in the solar atmosphere. Before the 60-inch telescope was erected the Snow telescope and the tower telescope were used to obtain high dispersion spectra of one or two stars. But the long exposure times required, 29 hours spread over four nights for Arcturus, prevented this work from being continued.

One other important investigation by Adams on the sun must, however, be mentioned, his classic investigation of solar rotation. His initial study with the Snow telescope was followed in 1908 by one using the tower telescope and the 30-ft. Littrow spectrograph. He confirmed his earlier result that higher levels in the sun's atmosphere showed a higher rate of rotation. He showed that the best fit for the equatorial acceleration was given by Faye's equation and that the equatorial acceleration was less marked for the higher levels in the atmosphere. He noted also that, when sunspot vortices were close to the limb, he got discordant results agreeing with a counter-clockwise rotation in the northern hemisphere.

With the completion of the 60-inch telescope, Adams took charge of the stellar radial velocity programme, working in particular with Joy, Sanford and Strömberg. The selection of stars to be examined was based on varying considerations; some brighter stars were chosen for comparison with those examined at Lick and Victoria with a view to a select list of standard velocity stars; some were chosen at the request of Kapteyn, mostly A and B stars of known proper motions; others chosen were faint dwarfs, spectroscopic binaries, long-period and Cepheid variables, stars with bright hydrogen lines. Altogether Adams was concerned with producing lists of radial velocities for some thousands of stars; before he ceased to be Director of Mt Wilson Observatory R. E. Wilson had commenced the preparation of the *General catalogue of stellar radial velocities* for 6739 stars of which Mount Wilson had contributed data for half.

Adams's early experience on radial velocity measures with Frost at Yerkes, his detailed study of the lines in the high dispersion spectra of the sun's disk and sunspots had enabled him to make the best possible use of the spectra of high dispersion that he obtained with the 60-inch and of the still higher dispersion available with the 100-inch telescope when it in turn became available.

In addition to the fuller studies that will be discussed later he published a whole series of short notes and papers on special points of interest in the spectra of individual stars; a great many of these were written jointly with Joy with whom he worked in the closest contact for years; in some cases he took advantage of the high dispersion spectra available to discuss points of interest in the special researches of others and he published joint papers with Shapley and Schwarzschild on Cepheids, with Humason on the red shift and on white dwarfs, with Greenstein on ν *Sagittarii*, a star with a nebulous envelope and a number of unexplained spectroscopic features, with Merrill on τ *Scorpii*, a star with discordant radial velocities, and with Russell and Mrs Moore-Sitterly on the calibration of Rowland's scale of intensities and on a method of analyzing stellar spectra for abundance of elements. Among his many papers on individual stars mention must be made of those in which he made valuable contributions to our knowledge of the following: α *Orionis*, γ *Cygni*, α *Boötis*, α *Canis Majoris*, α *Canis Minoris*, β *Capricorni*, ϵ *Aurigae*, σ *Ceti* near minimum, Boss 46, Boss 1517. Adams took advantage of the fine lines in the high dispersion spectra of α *Boötis* to make a determination of the solar parallax.

Adams published a number of papers on the spectra of novae, examining most of those that appeared from 1901 to 1936. At first he was inclined to attribute to some kind of dissociation the displacements of successive hydrogen absorptions, which he noticed to be in the ratio of 1:2:3. But when in the spectrum of *Nova Aquikae* 1918 he recognized an α *Cygni* absorption spectrum apparently displaced by a simple Doppler effect, he adopted the idea of an expanding shell or of a succession of shells as being the simplest explanation of the earlier spectra of a nova. With the great light-gathering power available

at Mt Wilson he was able with Pease to photograph a nova spectrum to a late stage in the fading of the star and could follow it through the nebular stage to the Wolf Rayet stage and explain puzzling changes in colour in terms of the bright bands present at any stage. He was also able to secure interesting details of the changes in the spectrum of *Nova Herculis* 1934 during its deep minimum and again during the subsequent slow fading. The discovery with Joy of coronal lines in the spectrum of *Nova (RS) Ophiuchi* 1901 at its outburst in 1933 should be mentioned as one of the many details of changing nova spectra that Adams secured for the use of the students of novae.

A dramatic element enters into the story of the next piece of research to be described. In 1918 Adams had attempted to examine the spectrum of the companion of Sirius, obtaining a spectrum at the 80-ft. focus of the 60-inch telescope. He secured almost certainly a line spectrum of the star identical with that of Sirius and he was clear that the colour indices of the two stars were the same. This led Eddington to predict that the companion was a white dwarf of extremely high density and to estimate a displacement of the lines of the spectrum to the red in accordance with the theory of relativity. In 1925 when the International Astronomical Union was meeting in Cambridge Eddington gave a lecture on relativity in the course of which he announced that he had just received a telegram from Adams: with the 100-inch telescope at the Cassegrain focus (135 ft. focal length), with a 40 minutes' exposure and a separation of the two stars of 10", Adams had secured a spectrum of the companion and had found, after allowing for the velocity of Sirius and an orbital correction, a displacement to the red of 21 km/s; Eddington's theoretical value was +20 km/s at $\lambda 4500$. Subsequently, using an improved orbit for the binary Adams altered his relativity displacement to +19 km/s, but the confirmation of the theory of relativity held good and has remained as, perhaps, the most striking instance of the skill of Adams in his study of high dispersion spectra obtained with the 100-inch telescope.

A further case where Adams had to pay careful attention to the delicate matter of slight changes in a spectrum in the presence of a strong overlying spectrum was in his search with Dunham for evidence of oxygen and water vapour in the atmospheres of Mars, Venus and Mercury. Confirming the results of St John and Nicholson they found no indication of either in the spectra of Venus and Mercury. In the case of Mars spectrograms taken at the coudé focus of the 100-inch gave for oxygen an upper limit of less than 1 per cent (and probably less than one-tenth of 1 per cent) of the amount in the earth's atmosphere above equal areas of surface, while for water vapour there was no evidence and an upper possible limit could be given as 5 per cent of the amount in the terrestrial atmosphere above Pasadena.

Reading Adams's papers one feels that one is reading a chapter in the history of practical astro-physics. To appreciate his greatest contribution to astronomy, the development of the method of spectroscopic parallaxes it is necessary to go back to the last century. Ritter and Homer Lane had worked out a theory of the condensation of a contracting mass of gas, steadily rising

in temperature to a maximum and then cooling again. Norman Lockyer followed these views in his stellar classification which contained red stars, now described as giants and dwarfs, at the two ends of the scale of evolution. He pointed out differences in the spectra of these two groups, but there was at that time no physical observations nor theory to support his views and, after a period of controversy, opinion hardened in favour of a simple line of evolution down the main sequence of stars from hot to cool stars. It was not long, however, before Hertzsprung and Russell, with increasing observational data of the brightness and parallaxes of stars found convincing evidence that the red or cool stars fell into two widely separated groups of giants and dwarfs; one or two tentative suggestions were made by Schwarzschild and Hertzsprung that differences in spectral details might be directly linked with a star's luminosity.

Meanwhile Adams in his detailed work on sunspot spectra and on the behaviour of 'enhanced lines' in the laboratory had been applying his spectroscopic experience to stellar spectra. Working at first with Kohlschütter, who had developed a quantitative method of stellar classification by inter-comparison of sharp and diffuse lines in the spectrum, he used material gathered together for a study of radial velocities of stars with large and small proper motions and stars of known parallax. They found that stars with small proper motions but of the same spectral type showed an obvious weakening in the continuous spectrum in the ultra-violet, rightly interpreted as interstellar absorption. But they also noticed that the stars with small proper motions—on the whole more distant and intrinsically brighter—had abnormally strong hydrogen lines (not to be ascribed for various reasons to interstellar absorption) while other pairs of lines showed relative intensities varying with proper motion. Applying these differences, measured on a simple quantitative scale, to stars of known parallax, they found simple relations between luminosity and spectroscopic differences. Thus the spectroscopic method of determining a star's parallax had been discovered: an examination of its spectrum gave its absolute magnitude; its apparent magnitude then gave its distance. The first joint paper appeared in 1914 and Kohlschütter was shortly afterwards recalled to Germany on the outbreak of World War I.

It fell to Adams in a series of valuable papers to develop fully this most important method of extending our distance probe into the surrounding stellar universe. He widened the scope of the method to include stars of various spectral types, he developed more accurate scales for the quantitative measurement of spectroscopic differences, on which the method entirely depended. As more and more accurate trigonometrical parallaxes were published by Yale, Leander McCormick, Yerkes and Mt Wilson Observatories he checked and rechecked his scales. The work of A. Fowler in the laboratory coupled with the theoretical researches of Saha, Milne and R. H. Fowler gave a physical meaning to the differences in the spectra which had been lacking for Lockyer's earlier work; it was indeed atomic dissociation

that was involved but not of the type Lockyer had postulated. Ultimately Adams published with Joy, Humason and Miss Brayton a catalogue of 4179 stellar parallaxes, essentially complete for the stars north of -20° in Boss's *Preliminary general catalogue*, with other stars from Kapteyn's selected areas, and a number of visual binaries, and stars with known trigonometric parallaxes, and large proper motions.

One further major contribution of Adams's must be mentioned—the study of clouds of interstellar gas. Beals had discovered in the spectra of several stars a doubling of the interstellar lines H and K of ionized calcium, and his observations had been confirmed with moderate dispersion by Sanford at Mt Wilson. Adams used high dispersion with the 100-inch telescope and found double or multiple interstellar lines for 80 per cent of the stars examined. He found that there were two classes of cloud: (a) with CN, CH, CH⁺ prominent and H and K of moderate strength; and (b) with H and K strong, CN and CH faint or absent and with neutral sodium, calcium, iron and potassium and ionized titanium well marked. In four cases four clouds were found moving with different velocities in the line of sight ranging up to 100 km/s. The larger and thicker clouds moved more slowly, the thinner and smaller clouds with the higher velocities; CN generally appeared with CH, which was found less frequently than CH⁺.

Reference has been made to the tasks that fell to Adams in the early days on Mt Wilson preparing for the Snow telescope, the tower telescope and the 60-inch stellar telescope. Similar responsibilities were largely his in the case of the 100-inch telescope for which the road up the hill had to be widened once more. Adams again had to take his share in the design of spectrographs for the 100-inch telescope; the change from prismatic to grating spectrographs with specially blazed gratings by Wood and Babcock, the application, with Dunham, of Schmidt optical principles, all called for his attention. From 1909 onwards he frequently carried the load of duties as Director. He has recorded the shock when with a party including Hale he attended the first optical test of the 100-inch Hooker telescope after the mirror had been mounted: six or seven images of Jupiter partly overlapping each other were to be seen. It transpired that the workmen had been letting direct sunlight fall upon the mirror; by 3 a.m. the same night the mirror had returned to normal.

After Adams became Director in 1923 he was responsible for the annual report of the Observatory, a document eagerly read at other observatories to find the latest discoveries and developments in work in progress. One of these reports gave his views of the organization of a research observatory and the need to apply to every suitable field all progress made in any one field. His co-ordinating power in the work of the observatory is reflected in the number of papers that he wrote jointly with the members of his staff.

Adams again took an active part in the realization of Hale's pipe-dream—the 200-inch telescope now on Mt Palomar. In 1926 he took Mr Thorkelson of the General Education Board of the Rockefeller Foundation up to the

Mt Wilson Observatory; it was Thorkelson's report that led to a subsequent visit of Wickliffe Rose and Thorkelson and to the grant of \$6000000 from the Rockefeller Board. Adams was also involved in the further negotiations that led to the approval of the final scheme. He always as Director attended the Observatory Council, which was the executive body making the final decisions upon major questions affecting the 200-inch telescope. There were two other committees, which reported to the Observatory Council—a policy committee, dealing with such general questions as the functions of the telescope and the auxiliary instruments, and a construction committee, dealing with technical questions of design and engineering. In 1934 Adams must have revived old memories of the Toll Road on Mt Wilson when he drove up the Nigger Trail on to Mt Palomar to assist in the selection of the site for the new observatory. The dome, details of the mounting, the drive and control system were all finished or settled before Adams retired in 1946, a retirement delayed for two years on account of World War II.

He did not sever his long connexion with the Observatory, continuing to work as a Research Associate of the Carnegie Institute of Washington, 1946-8, and of the California Institute of Technology, 1947-8. He thus maintained his link with the Mt Wilson Observatory throughout its whole history from 1904 to 1947 when it was merged with the Mt Palomar Observatory under the joint control of the Carnegie Institution of Washington and the California Institute of Technology. It was natural that he should be the guest of honour at the Jubilee Dinner at Pasadena on 20 December 1954. He had also naturally been chosen by the National Academy of Sciences as one of the delegates to the Royal Society's celebrations of the Newton Tercentenary in 1946. He gave an address on 'Newton's contributions to observational astronomy' illustrated by the great developments that had sprung from Newton's discovery of the spectrum and invention of the reflecting telescope. Adams continued active to the end, a very typical paper 'Notes on the Shell lines and the radial velocity of Alpha Orionis' appearing in the last number of *The Astrophysical Journal* before his death.

Many honours came his way. He was awarded the Gold Medal of the Royal Astronomical Society in 1917, the Draper Medal of the National Academy of Sciences in 1918, the Prix Janssen of the Société Astronomique de France in 1926, the Bruce Medal of the Astronomical Society of the Pacific in 1928 and the Janssen Medal of the Paris Académie des Sciences in 1935. He was an honorary doctor of Chicago, Columbia, Dartmouth, Pomona, Princeton and Southern California. He was a Foreign Associate or Member of the Royal Astronomical Society, 1914, the Royal Swedish Academy, 1935, the Institut de France, 1945, and the Royal Society, 1950. He was a member of the National Academy of Sciences and of the American Philosophical Society; he was President of the Astronomical Society of the Pacific, 1923, of the American Astronomical Society 1931-4 and of the Pacific Division of the American Association for the Advancement of Science in 1929.

He was a Vice-President of the International Astronomical Union, 1935-48, and acted as General Secretary 1940-5 to keep the Union alive during the war years, when the General Secretary, Professor Oort, was cut off from participation in international activities. He was also the first President of the Commission of the Union on the Spectral Classification of Stars, President of the Commission on Radial Velocities and a member of five other commissions. In his connexions with the Union he showed himself, as ever, a valuable adviser, willing to co-operate and help when called upon.

Adams was twice married. His first wife, Lilian M. Wickham, died in 1922. His second wife, Adeline L. Miller, survives him with two sons. He died on 11 May 1956 of a cerebral thrombosis after a short and painless illness.

F. J. M. STRATTON

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J. V. Amers.

GLEB ANREP

1891-1955

LIFE

GLEB (VASSILIEVITCH VON) ANREP was descended from a famous Westphalian family with a proud history of high military distinction which can be traced back to the tenth century. Field Marshal von Anrep was leader of the Livonian Order of Knights who built an outpost of Christianity on the shores of the Baltic. General von Anrep came to St Petersburg at the invitation of Peter the Great and founded the Russian branch of the family in the eighteenth century.

Professor Vassili (Basil) von Anrep (1854-1925) was the first member of this family to prefer science to arms. He was born in St Petersburg and studied law in the university there for a year, but he soon tired of academic legal arguments and entered the Medical Academy where he was a brilliant student. He spent two years studying pharmacology in Leipzig, where he described the local anæsthetic action of cocaine and recommended its use in medicine four years before its introduction by Koller. He returned to Russia to be professor of pharmacology, first in Kharkov and then in the Medical Academy in St Petersburg. At the invitation of Prince Oldenburgski he founded the Institute of Experimental Medicine and directed it for several years. At the command of the Emperor he then founded a medical institute for women doctors, which was needed because Moslem women refused to be treated by men. His public duties interfered with his researches and eventually he devoted himself entirely to the reform of education. He held high administrative posts in connexion not only with medicine but also with education in general, and nearly succeeded in introducing compulsory education for all Russians. In the first world war he was at the head of the Russian Red Cross. At the outbreak of the revolution he was thrown into prison, but was released after six months and since the family had come from the Baltic he managed to obtain a Latvian passport. He arrived in London in 1919, stayed with his younger son Gleb for a few years and then left for Paris, where he died in 1925.

Gleb v. Anrep's contact with science started early, since he was born (on 23 September 1891) in the Institute of Experimental Medicine, St Petersburg, where his father was director. He was the youngest of four boys, of whom the elder two were half-brothers by his mother's first marriage, and the third is Basil v. Anrep, who is famous for his work in mosaic, which may be seen

in the National Gallery and the Tate Gallery in London, and in many other places all over the world.

Gleb was a solitary child, interested in mysticism and fond of discussing abstract matters with grown-up people. He never liked sport, but was devoted to music. He studied Latin and Greek in a gymnasium and was awarded a gold medal, but his mind was chiefly occupied at this time by what was called in Russia 'the Quest for Truth'. He sought to penetrate the mysteries of creation and was encouraged by an aunt to follow the teachings of a bizarre prophet who interpreted modern events as the fulfilment of prophecies to be found in the Bible. He fervently studied the scriptures in the hope of finding new meanings in obscure passages and discussed religion with bishops, but did not find happiness or satisfaction in this quest.

Under his father's influence, when the time came, he entered St Petersburg University, as a student in the Faculty of Science, but without losing contact with his mystical friends who were devoting themselves to the study of spiritualism. The rational atmosphere of the science faculty must have produced conflicts in his mind and he failed in his first examination. This was a great blow to his pride, and he became so nervous that he was sent to Germany; he returned after a few months, sound in mind and body and prepared to devote himself to science. He entered the Medical Academy where he met Professor I. P. Pavlov and became his devoted pupil and friend. He abandoned mysticism for ever and spent less time on music, although as a young man he had shown real talent as a pianist and even as a composer. While he was still a student in 1912, 'Ivan Petrovitch' sent him to visit Starling in London in order to demonstrate to him the effect of vagus stimulation on the secretion of pancreatic juice and to learn the method of preparing secretin. He already spoke tolerable English and became fluent during that and two similar visits in the next two years. In later years he was a master conversationalist in the English language, and also spoke French, German, Italian and Arabic.

The third visit to London was cut short by the outbreak of war in 1914 and he hastened back to Russia with his brother Boris, who had been working on mosaics in London. He qualified as a doctor in 1915 and immediately joined the army and took an active part in the war. He was twice wounded and was awarded the St George Cross, which could only be won by feats of outstanding bravery. The company in which he served as a doctor had been almost completely surrounded and all the combatant officers were killed. Anrep took command and led his company by day and night through swamps and forests to safety.

When the revolution broke out in 1917 he joined the White Russians under Denikin; he was captured but escaped again and fought on till the end. He came to London with his first wife, Olga Volkova and was appointed assistant in the department of physiology at University College London, under Starling.

Years afterwards, in the preface to his *Lane Lectures* (1936) Anrep wrote

‘Pavlov’s personality and that of my later teacher, Starling, had equally dominating influences upon my development as a physiologist, and I think this preface is the most fitting place in which to express my deep regard for these two gallant men of science. Strikingly different in their general temperament, they were alike in the completeness with which they experienced the unbounded joy of scientific discovery. The Pavlov of digestion was a physiologist of the old school; the Pavlov of conditioned reflexes one could almost say was a physiologist of the future; and Starling was a physiologist of the transition stage between the old physiology of observation and the present physiology of scientific analysis. I humbly hope that in my work . . . I have been able to live up to the high principles which guided my two teachers.’

Anrep threw himself with enthusiasm into his new life in London. He was a skilful operator and soon learned to make heart-lung preparations quicker than anyone else. He was a vivid teacher and an accomplished lecturer. He won the Sharpey-Schäfer prize, and the W. Mickle prize and gave the Sydney Ringer lecture. His only son, John, was born in London in 1922, educated in England and married an English wife and now works in a laboratory as an industrial research worker in Manchester.

In 1925 Gleb Anrep moved to Cambridge as lecturer and acquired British nationality. He was always proud of this and clung to it later in Egypt when it might perhaps have been wiser to change his nationality again. ‘We British’, he said, ‘do not do that sort of thing.’ He was elected F.R.S. in 1928 at the early age of 37.

He was never happy in Cambridge, where his wife died suddenly. He must have welcomed the invitation to go to Kasr el Aini, Cairo as Professor of Physiology and accepted this offer after visiting Egypt as external examiner in June 1930. He married a distant cousin Dina von Anrep and they went out together in 1931, but they were not happy for very long and the marriage was eventually dissolved.

The warmth and colour of Cairo suited him and he learned to speak fluent Arabic. He understood the Egyptians better than most foreigners and was fond of visiting the bazaars and drinking coffee with the merchants for hours on end until he had got what he wanted at a low price.

He bought an open two-seater Ford car with a dicky and drove it all over Egypt for nearly a quarter of a century. He made exciting trips across the desert, and did not always obey the rule which says that a single car must not be separated from other cars. One day, he became impatient and drove ahead until he was lost in the desert and got stuck in sand 100 miles from help. He spent the whole of one day, from sunrise to sunset, getting his car on to firm ground. He used to say that there were thirteen ways of getting out of sand and that he tried them all.

He was an enthusiastic philatelist and he won a prize for his collection of Egyptian stamps which was said to be one of the finest in the world. He had an arrangement with various postal officials in Cairo through which he got early information about mistakes in printing and other new kinds of stamp

and published accounts of his findings in philatelic journals. He used to boast that some kinds of stamp were known as the Anrep variations. He had one stamp which even the King of Egypt did not have and he kept it concealed in a bottle in his laboratory.

He married Ida, the widow of an Italian in Cairo and lived very happily with her for a few years, but she died as the result of a bite from a mad dog and he had a bad nervous breakdown for which he was treated in hospital. As the result of a dream, his wife's sister Annie Wieninger came out from Italy to Cairo to look after him and eventually married him. She is an anthroposophist and a follower of Rudolf Steiner and she survives him. Anrep had no sympathy at all with the philosophical interests of his wife and did not hesitate to say so. The mysticism of his early years had evidently not been merely forgotten; it had been repressed.

Anrep was undoubtedly happy in Egypt and the main cause of his happiness was that he was head of a well-equipped laboratory with good facilities for research. He hoped to create in Egypt a school of physiology like that of Pavlov or Starling and he succeeded in inspiring his staff with love and respect and enthusiasm for scientific work. They often worked all day and then worked on all through the night and Anrep was always ready to do his share of the duller tasks as well as providing most of the drive and most of the original thought. When a series of experiments had given a clear result he would say 'It is time to think' and summon his colleagues together for discussions which might last hours and might last days. 'It is better to criticize our experiments if we can before others do so.' When new discoveries were made the whole team celebrated the occasion by spending an amusing night in the town. His colleagues soon learned that when they were asked 'How is life?' the proper reply was an account of the results of their experiments.

Anrep believed that the only thing that really mattered in the education which might be acquired by attending a university was enthusiasm for research. He used to say that it was only by taking an active part in a discovery that a real knowledge of science could be acquired, and he persuaded the University of Cairo to insist that a thesis should form part of the examination for any postgraduate degree in medicine; in surgery a thesis would exempt the candidate from a part of the examination. He believed that anyone who could do research was bound to be able to excel as a teacher provided that he would take the trouble. He himself was a first-class lecturer. He spoke fluently with great vigour and never lost the thread of his theme. At the beginning of every lecture he invited questions and derived obvious pleasure from answering intelligent ones.

He spent time in teaching his assistants how to teach. He told them that the best lectures contained few facts, but that these facts were presented in an integrated story. His teaching was not systematic, but he discussed physiological problems with his students and left them to work out the details themselves.

Some students of course were more interested in other things and apt to

behave badly during lectures when there was excitement in the streets. On one occasion, he interrupted his lecture to say that he hoped that those students who wanted to wave flags would leave the theatre, so that those who were interested in physiology might hear what he had to say. This suggestion was thought to be a good one and about half the students went out. On another occasion there was fighting in the medical school and Anrep was injured.

In 1952, Anrep was a victim of the nationalist rising under General Neguib. He lost his position in the University and was not allowed to continue his scientific work or even to visit his laboratory. For the second time his work had been interrupted by a revolution.

He was invited to attend meetings arranged by the Ciba Foundation in April 1955 in London, and to take the chair at a public discussion on histamine, but he wrote to say that he was no longer in touch with recent work and did not feel justified in accepting the invitation. When pressed he agreed to come to London for these meetings, but he had a heart attack and then died ten days later in a second attack on 10 January 1955. His death must have been due to disease of the coronary vessels, about which he knew more than any other man.

WORK

Anrep's first original research was carried out in London and published when he was 21. Bayliss had found that a rise of blood pressure, due to stimulation of the splanchnic nerve or of a sensory nerve or asphyxia, was followed by a second rise, and concluded that the blood vessels had the intrinsic property of contracting in response to a stretch. At the time when this work was done, however, it was not known that adrenaline might be released from the adrenals by the splanchnic nerves. Anrep repeated these experiments after adrenalectomy, and recorded not only the blood pressure, but also the volume of a limb and the rate of the heart. He was thus able to show that the second rise of blood pressure was due to the release of adrenaline. Bayliss's conclusion about the response of plain muscle to a stretch was ill-founded, but there are now other reasons for believing it was correct.

Anrep also observed that in the absence of the adrenals arrest of the circulation led to vasodilatation and obtained some evidence that this was due to the release of metabolites. This was not the first evidence of reactive hyperæmia, but it showed the phenomenon in a new and clear way and aroused Anrep's interest in a problem to which he returned over twenty years later.

Anrep had come to London to demonstrate Pavlov's discovery that stimulation of the vagus caused a flow of pancreatic juice. Pavlov had also observed a decrease of flow in certain conditions, and concluded that the vagus contained both excitator and inhibitor fibres to the pancreas. Anrep confirmed the facts but not their interpretation. He put the pancreas in a plethysmograph and found that the decrease of flow was accompanied by an

increase in the volume of the gland. After excluding vasomotor effects, he concluded that the decrease of flow was due to constriction of the ducts and this conclusion is now generally accepted, although Pavlov did not like it and continued to believe that better evidence of the existence of inhibitor nerves to the pancreas would eventually be found.

Anrep played an important part in introducing conditioned reflexes to the western world. In the spring of 1924, Pavlov gave a series of 23 lectures in the Military Medical Academy in Petrograd in which he summarized the work of nearly 25 years on the activities of the cerebral hemispheres. These lectures were taken down by a stenographer, but the result was not entirely satisfactory and Pavlov devoted his leisure during more than 18 months to their revision. The Royal Society arranged for the publication of an English translation of the resulting book, and Anrep was made responsible for this. In the preface of this book Pavlov expresses his gratitude to the Society and to his 'young friend Dr Anrep, formerly an active collaborator in the work described in these pages'. This was the first systematic account of conditioned reflexes in English and much thought was needed to find suitable equivalents for a large number of new technical terms which had been invented in Russia to describe new ideas. Pavlov had also written another book called *Twenty years of objective study of the behaviour of animals* which was translated into English by W. Horsley Gantt and published in America in 1928.

Anrep himself published five papers on conditioned reflexes—three in Russian and two in English. The first English paper, published in 1920, dealt with pitch discrimination in the dog. Various Russian workers had found that dogs had very acute powers of hearing and that some of them had a sense of absolute pitch which enabled them to discriminate notes differing by a quarter of a tone. Johnson, in a paper published in *Behaviour Monographs* in 1913 had failed to find any such thing, but his methods were very different. Anrep confirmed the Russian results and discussed the reasons for the difference of conclusion. He took the opportunity to describe the Russian technique in some detail and to draw attention to the advantages of experiments on saliva in which all extraneous stimuli are eliminated over experiments in which a dog is released from a cage and looks for food in alternative places.

His other English paper deals with data which he had already published in Russia. The conditioned stimulus was a tactile stimulus applied to the skin of one spot on the thigh. Other parts of the body gave lesser responses and the size of the response depended on the distance from the original spot. This irradiation of the reflex depended on the pause between the conditioned stimulus and the unconditioned stimulus. One remarkable feature of these results was that symmetrically situated points on the right and left side of the dog gave identical responses, even though the reflex was only formed on one side.

Much of Pavlov's work on conditioned reflexes depended on records of the flow of saliva and when Anrep settled down to work in England he studied the metabolism of salivary glands in some detail. Between 1921 and 1924 he

published half-a-dozen papers on this subject with various collaborators in the *Journal of Physiology*. He measured the glucose consumption of resting glands and showed that it was increased during activity. This increase stopped when secretion stopped and did not continue during the recovery period as did the increased oxygen consumption. Moderate doses of atropine might suppress secretion completely without suppressing the consumption of glucose. After prolonged stimulation the gland took several days to recover. The rate of recovery was not affected by the administration of atropine, but was actually accelerated by excision of the superior cervical ganglion, from which sympathetic nerves run to the gland. This result gave no encouragement to those who believed that the sympathetic nerves had a 'trophic' influence on the salivary glands. All this work was sound enough, but it cannot have given much satisfaction to Anrep's fondness for dramatic results.

In 1925, Anrep and Starling published the results of an investigation of some of the factors controlling the rate of the heart. According to Marey's Law an increase of blood-pressure was known to cause a slowing of the pulse. Anrep and Starling studied the mechanism of this response by perfusing the head and neck of one dog by means of a heart-lung preparation made from another dog. These experiments seemed to show that the heart rate was not affected by changes of pressure in the head, but depended on the pressure in the rest of the body. At the same time, Heymans in Ghent was studying the same phenomenon by a similar technique except that the circulation to the head came from a whole dog instead of from a heart-lung preparation. He came to exactly the opposite conclusions and both groups of workers must have felt anxious to discover the truth as soon as possible. Starling was a sick man and Anrep worked with Segall, using what was essentially two heart-lung preparations, one of which pumped blood to the head and neck from which the vagus of the second heart-lung preparation ran. Heymans also modified his technique and eventually both groups discovered that both groups had been right in their positive findings and wrong in their negative findings. Reflexes arise both in the arch of the aorta and in the carotid sinus and the heart may be slowed by either reflex. Heymans recognized the importance of the carotid sinus and continued the work which led to the award of the Nobel prize to him in 1938. Anrep was diverted to work on the coronary vessels which occupied most of his attention between 1926 and 1933.

He devoted much work to the study of the rate of flow of blood through these vessels during the different parts of the cardiac cycle and introduced new techniques for this purpose. The changes in electric resistance in a hot wire due to movements of air over it had been used in war to locate guns by recording the arrival of sounds, and by A. V. Hill to follow the propagation of pulse waves in arteries. Anrep calibrated this hot wire anemometer and used it to follow the flow of air through tubes connected with reservoirs of blood and so got records of quick changes.

When one of the coronary vessels was perfused at constant pressure the rate of flow was much decreased during the early part of systole and Anrep

attributed this to the direct mechanical effect of the contraction of the cardiac muscle. Under normal conditions this increased resistance of the heart to the flow of blood through the vessels during systole must be to some extent overcome by a simultaneous increase of the pressure in the aorta, but direct measurements showed that even so the flow of blood was diminished during systole, and in strongly beating hearts Anrep sometimes observed a reversal of the flow; the cardiac muscle appeared to force the blood back into the coronary artery. The reality of this phenomenon was confirmed by inserting a valve, which prevented the backward flow and thus increased the net forward flow of the blood.

Similar methods were applied to the rate of outflow of blood from the coronary sinus during the different stages of the cardiac cycle. He found that the blood came out in three waves corresponding in time with auricular systole, with isometric ventricular systole and with the contraction of the ventricle. The last wave was attributed to the mechanical effect of the contraction of the muscles forcing blood out of the heart.

These results were disputed, particularly by Hochrein who believed that systole favoured the flow of blood through the coronary vessels. Anrep (1936) was not convinced and said that 'the observations of Hochrein were due to some grave errors in his technique'. However, he also said (1933) 'We refrain from any polemical discussion of this problem, as we prefer to subject the question to a still further experimental test.'

Since the hot wire had been criticized, he recorded the quick movements of the blood by photographing the movements of a drop of mercury in a tube. When the coronary artery was perfused at constant pressure the results with this new apparatus agreed satisfactorily with the results with the hot wire, but when the artery received a pulsatile flow of blood from the aorta the droplet of mercury showed not only the expected movement towards the heart during diastole, but also a small movement in the same direction during systole. A lesser man might have supposed that Hochrein was perhaps right after all, but Anrep did not think this, and came to the conclusion that the results were due to the filling up of the larger coronary arteries and did not indicate a real flow of blood to the heart. He confirmed this theory by injecting lycopodium seeds, which blocked the capillaries completely, so that the flow of blood through the heart stopped, but the heart continued to beat for a short time and during this time the small movement of the mercury droplet toward the heart during systole continued to take place.

With the help of his very competent technician, Mr F. W. King in Cairo, he devised a machine which drove the heart at constant speed and could be made to clamp the flow to the coronary vessels for brief intervals at any stage of the cardiac cycle. He showed that clamping during diastole decreased the total flow, but clamping during systole had little or no effect on the total flow. This confirmed his view that there was little or no flow during systole.

In the face of all this evidence few continued to doubt that Anrep was correct in his views on this point; but it was said that much of his work had

been done with heart-lung preparations and that conditions were different in the whole animal. Anrep applied similar techniques to the heart under more physiological conditions and got similar results.

These academic questions of the quick changes in different parts of a single cardiac cycle have no obvious direct relation to the question of the total amount of blood flowing through the coronary vessels per minute, and this is what matters for the welfare of the heart. Anrep was especially interested in this because of its relation to diseases of the coronary vessels and devoted much work to it. He showed that in the isolated heart-lung preparation the total flow was directly dependent on the mean pressure in the aorta and was not affected by changes in the pulse pressure. It was not much affected by the frequency of the beat or by changes in the output of the heart. When the heart was in its normal place in the body the coronary flow was dependent on vasomotor reflexes. The results of previous workers were sometimes contradictory, but Anrep's experiments led him to definite conclusions. Stimulation of the vagus constricted the vessels. Stimulation of the sympathetic dilated them. A rise of pressure in the carotid sinus constricted the coronaries through a reflex and so tended to counteract the rise of flow due to increased aorta pressure seen in the heart-lung preparation. An increase in the output of the heart caused reflex coronary dilatation which Anrep believed to be due to a mechanism like that which controlled the heart rate, according to the work of Bainbridge. He got evidence that this effect was due to a decrease of the tonic vasoconstrictor effect of the vagus.

In connexion with this work Anrep did some of his most dramatic experiments. It was possible in Egypt to revive human hearts very soon after death, and in his Lane Lectures (1936) he gives a brief account of a number of experiments with human heart-lung preparations, which continued to beat well for over four hours. He perfused the coronary vessels at constant pressure, collected blood with a Morawitz cannula in the coronary sinus, and so showed that many of his conclusions about dogs' hearts were also true about human hearts. He showed that the human coronaries were dilated by adrenaline, caffeine, sodium nitrite, papaverine and adenosine and constricted by posterior pituitary extract.

In the same lectures he described experiments on dogs, in which he first established collateral circulation between the surface of the heart and the triangularis sterni muscle, and then eventually tied both the right and left coronary arteries. Some of these dogs ran about the laboratory in an apparently normal state for months after their operations. This work was inspired by an autopsy on an Egyptian in whom a similar collateral circulation had developed without the aid of surgery, and who had been able to do heavy work in spite of severe coronary disease. When he visited America in 1935 to give the Lane Lectures Anrep met Dr Beck of Cleveland who had done similar experiments, and who later carried out a similar operation on man for the relief of coronary disease. This operation was popular for a time, but the benefits derived from it were not very obvious and it is seldom done now.

In the last of the five Lane Lectures Anrep describes investigations of the circulation in voluntary muscles by the methods which had been successful in the study of the circulation in the heart. Gaskell had found in 1877 that tetanic contraction of a muscle had three effects on the venous outflow—an initial increase, which was attributed to compression of the blood vessels, a decrease of flow during the contraction and a second increase which was thought to be due to the release of vasodilator substances. Rein of Göttingen had studied this same problem by recording the flow through the artery with his *thermostromuhr*, and came to the conclusion that during the actual contraction of the muscle the flow was not decreased, as Gaskell's results seemed to show, but increased; this was consistent with the observations of Spalteholz on the anatomy of the circulation. Anrep criticized Rein's technique on the grounds that his apparatus was slow to respond and apt to give misleading results, and that the flow to the whole leg was recorded so that the results may have been complicated by changes in the flow through the skin. It was natural to expect that the contraction of voluntary muscle would have the same effect on the flow as the contraction of cardiac muscle and Anrep studied this question experimentally by the same methods. He ligated all the blood vessels except those going to one muscle and perfused that muscle at constant pressure recording the arterial inflow with a hot wire. He thus showed clearly that the immediate effect of contraction of voluntary muscle was an increased resistance to blood flow, whether the muscle was stimulated directly, or through its nerve, or reflexly. Rein said that such results were due to the fact that the stimulus which was applied to the motor nerves also stimulated the sympathetic vasoconstrictor nerves. Anrep marshalled much evidence against this view—the effect was too rapid; it was not antagonized by ergotoxine; it was easily abolished by curare; it occurred when the muscle contracted reflexly; and lastly, it still occurred when the influence of the vasoconstrictor nerves was excluded by degenerative section.

These results justified the conclusion that the direct effect of contraction of voluntary muscle was the same as the direct effect of contraction of cardiac muscle and he proceeded to study the secondary vasodilatation. A Dewar flask was fixed over a dog's leg, and taps were provided, so that blood could be made to flow into this flask either from the artery or vein. It could then be driven by air pressure into the artery, while the outflow from the vein was recorded with a hot wire. Contraction of the muscle was followed by hyperæmia and the release of vasodilator substances, which could be detected by reperfusing the venous blood. This experiment was, however, only successful when the flow to the muscle was controlled with a clip; otherwise the flow increased so much that the vasodilator substances were too dilute for detection. These vasodilator substances were also detected in experiments where the artery was temporarily clamped when the muscle was stimulated, and by altering the relation between the clamping and the contraction of the muscle it was shown that the vasodilator substances were stable in the muscle for a few minutes at least. Rein had come to the conclusion that vasodilator

substances were formed during the relaxation of the muscle, but Anrep obtained evidence against this view and believed them to be liberated during the contraction of the muscle. He showed, for example, that five brief tetani followed by five relaxations had no more effect than one tetanus of the same total duration and followed by only one relaxation: when blood was forced through contracting muscle, vasodilator substances appeared although relaxation did not occur till after the blood was collected.

It has long been supposed that vasodilatation of the type studied in these experiments is due to acid metabolites such as CO_2 and lactic acid. The discovery of more active vasodilators such as histamine suggested another possibility, since it was difficult to believe that such substances played no part at all in the local adaptation of the blood vessels. An opportunity of investigating this possibility arose at the right moment since Barsoum and I discovered the normal presence of histamine in blood and worked out a method of estimating it in 1934 in Cairo, while Anrep was still engaged in his experiments on reactive hyperæmia. Anrep and I started work together and I have always regretted that my return to London prevented us from doing much. G. S. Barsoum was transferred from the Pharmacological Department to the Physiological Department and was Anrep's chief collaborator during the last 17 years of his active life.

Papers appeared which presented evidence that histamine was continuously released from both voluntary muscle and cardiac muscle and that the rate of release was increased when the muscles did work. Workers in other countries have failed to confirm this particular result and in spite of much more work in Egypt on this problem the reason for this failure is still unknown.

During the next ten years Anrep and his collaborators made three major contributions to our knowledge of the behaviour of histamine in the body.

In 1939, they discovered that histamine was liberated from voluntary muscles by curare. This drug has been used in experiments on stimulated muscles to exclude the possibility that the histamine came from the nerves. Control experiments showed that curare was itself effective and this provided the first example of the group of drugs which were later studied by MacIntosh and Paton and became known as the histamine liberators.

In 1941, they published evidence of the enormous increase of histaminase in the circulating blood which occurs in women during pregnancy. Some evidence that histamine was destroyed more rapidly by pregnant blood than by normal blood had been published by others, but the work of Anrep's school clearly showed its importance. Their results were published in Egypt and owing to the war their paper did not reach the laboratories of Europe and America. A full account appeared in the *Journal of Physiology* in 1947.

In 1944, Anrep and his colleagues published an important paper on the excretion of histamine in urine. They devised a method of pharmacological purification involving adsorption of the histamine on charcoal and elution with acid alcohol. They tested such extracts of urine on the guinea-pig's ileum, and showed that the histamine which they contained was partly free

and partly conjugated as a pharmacologically inactive compound which could be converted to free histamine by acid hydrolysis.

In order to get evidence of the meaning of this, they visited the zoo and collected urine from the animals living there. They found that the urine of carnivores (lion, tiger, leopard and cheetah) contained a high concentration of conjugated histamine (more than 3000 μg of the base per litre) and that the urine of herbivores (rabbit, horse, donkey, water buffalo, elephant, camel, llama and anthropoid apes) contained little histamine (less than 70 μg per litre) and that this histamine was free. A group of mixed feeders (man, cat, dog and rat) gave intermediate and variable results. The collection of the material for these experiments was not always easy and it is understood that a giraffe put its urine everywhere but in the right place.

Direct experiments on rats and dogs showed that the urinary histamine depended on the diet. When meat was given by the mouth, large amounts of conjugated histamine appeared in the urine. When histamine itself was given by the mouth about 5 per cent of it appeared as conjugated histamine in the urine. When histamine was injected, on the other hand, free histamine appeared in the urine and there was no increase in the excretion of conjugated histamine.

These results suggested that conjugation occurred either in the liver or in the intestine and later experiments, which seem to have been published only in the proceedings of the Eighteenth International Physiological Congress (1950) provided evidence against the first of these possibilities and in favour of the second. It was known that conjugated histamine had a low renal threshold and it was shown that when the renal vessels were occluded the administration of histamine by the mouth led to the appearance of large amounts of conjugated histamine in the blood plasma and no corresponding increase of free histamine. When histamine was infused into the mesenteric vein, conjugated histamine did not appear in the blood as would have been expected if the liver played much part in the conjugation. It was also shown that the inhibition of the intestinal flora with sulphonamides prevented the excretion of conjugated histamine in the urine, and it thus seemed likely that the conjugation was due to bacteria in the intestines. The meaning of the results obtained with meat is still obscure, since the amount of conjugated histamine appearing in the urine is sometimes greater than the amount of histamine originally present in the meat.

Most of these results have now been confirmed several times and their importance is widely recognized. Histamine is acetylated in the body and acetylhistamine has all the properties of Anrep's conjugated histamine. The site of its formation is still under discussion and the possibility of other forms of conjugation is not excluded. The amount of histamine appearing in the urine is comparatively small, but through the work of Anrep and his colleagues it has become possible to use it as an indirect index of histamine metabolism.

Anrep's last two papers in the *Journal of Physiology* appeared in 1953 and provided more evidence of the part played by the liver in histamine meta-

bolism. Estimates of the amount of histamine in arterial blood, and blood from the portal and hepatic veins of anæsthetized dogs showed that histamine was normally added to the blood by the intestines and removed by the liver. When the histamine in the systemic blood was raised by the injection of histamine or curare, both of these tissues removed histamine from the blood. Curare liberated histamine from the muscles, but not from the liver; bile salts, on the other hand, liberated histamine from the liver, but not from other tissues. When the bile duct was obstructed the blood histamine started to rise on the third day, reached a high value between the ninth and twelfth days and then declined before death. This effect appeared to be due to the effect of bile salts on the liver. These results show that Anrep never lost his originality and his ability to carry experiments through to stimulating conclusions.

During the last ten years of his life Anrep published half-a-dozen papers on *Ammi visnaga*. This umbelliferous plant grows wild in Egypt and has long been used, in the form of a decoction, as a diuretic and to promote the removal of renal calculi. Karam Samaan showed that extracts of it caused relaxation of plain muscle, and studied the active principles responsible for this effect. Anrep's interest had been aroused (1945) because an Egyptian member of his technical staff was using this drug for renal colic and it seemed also to relieve pain due to angina pectoris. Anrep and his colleagues found that it caused prolonged dilatation of the coronary vessels of a dog and suggested that it might be used clinically in the prophylaxis or treatment of angina pectoris. Gorgi Barsoum travelled abroad and demonstrated the effect in heart-lung preparations and various pharmaceutical firms became interested. The alkaloid khellin was recognized to be the main active principle. Its structure has been determined (2-methyl-5,8-dimethoxyfuranochromone) and it is available for general use. Favourable reports have been published, but critical tests have so far failed to establish its value in angina pectoris. It may prove of value in the treatment of asthma.

Anrep's work brought him widespread recognition. He was M.D. (St Petersburg), D.Sc. (London) and M.A. (Cambridge). He was a Member of the Physiological Society (1913), Member of the Institute of Egypt (1933), Fellow of the Biological Society, Vienna (1934), Fellow of the Roumanian Academy of Medicine (1934), Honorary Fellow of the Harvey Society, New York (1935), Fellow of the Cooper Lane Medical Association, St Francisco (1935) and Fellow of the Medical Association of Latin American States (1935).

Such honours meant little to him. He had no respect for the conventions of society and was happy only in his laboratory, driven on by an almost religious passion for the discovery of the truth.

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J. H. GADDUM

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Alfred S. May

OSWALD THEODORE AVERY

1877-1955

OSWALD THEODORE AVERY, Emeritus Member of the Rockefeller Institute for Medical Research, died in Nashville, Tennessee, on 20 February 1955 at the age of 78. His name calls to mind a slender, affable, neatly dressed man with large brilliant eyes and a smile of welcome. Very early in his career Avery came to be known as 'The Professor'—or more familiarly 'Fess'—and he retained this nickname throughout his life even though he eventually gave up lecturing almost entirely. He was indeed 'The Professor' by virtue of his gentle wisdom in counsel and of his art as an expositor of science—always spicing his performance with mimicry, pithy remarks, picturesque analogies and verbal pyrotechnics.

The appellation 'The Professor' symbolizes the extroverted, engaging aspect of Avery's personality, which made any contact with him such an enjoyable and rewarding experience. To a few of us who saw him in everyday life, however, there was often revealed another aspect of his personality, less obviously pleasurable but with a more haunting quality. We remember a brooding forehead that appeared too heavy for the frail body, a gaze focused inwardly as if unconcerned with the surrounding world, a melancholy figure whistling gently to himself the lonely tune of the shepherd song in 'Tristan and Isolde'. Avery's response to the demands of society was made up of these two conflicting attitudes. The telephone would ring announcing a visitor, or bringing an invitation to some social gathering. The reaction was immediately one of joyful acceptance, or of profuse regrets couched in the most flattering language in case of refusal. Then, as the conversation ended, the smiling mask was suddenly dropped. From behind it appeared a tired and almost tortured expression. The telephone was pushed away on the desk as a symbol of lassitude and of protest against the encroaching world.

An acute need for privacy, even if it had to be bought at the cost of loneliness, conditioned much of Avery's behaviour. He hardly ever spoke of his private life, or of his past, unless the events had some bearing on his scientific activities. His conviction that scholarly achievements should be evaluated on their own merit, without concern for the personal affairs of the scholar, was reflected in the obituary notice that he published in 1944 on the occasion of K. Landsteiner's death. The editor of the journal in which the notice was to be published requested that the account be supplemented with details of Landsteiner's personal and family life, but Avery firmly refused with the statement that these details would not contribute to the understanding of

either the achievements or the mental processes of the scientist. The same view had once been expressed by Claude Bernard 'A great man is not great when he goes to bed, gets up, sneezes, etc, but only when he writes, thinks . . . It is in these moments that . . . we can reach him through his works. We had better ignore the rest; it does not add anything to the man.' I shall respect this attitude and present only the barest outline of Avery's private life.

Avery was born on 21 October 1877 in Halifax, Nova Scotia. His father, a clergyman, moved in 1887 to New York City where he undertook missionary work in the Bowery. Although Avery did not become a U.S. citizen until 1917, his entire life from the age of 10 to 70 was spent in New York. He attended Colgate University from which he received an A.B. degree in 1900. He concluded his formal medical education at the College of Physicians and Surgeons of Columbia University in 1904. After early work in bacteriology, at the Hoagland Laboratory in Brooklyn, he joined the Rockefeller Institute in 1913, and was appointed to full membership of this Institute in 1923. After becoming Member Emeritus in 1943, he continued his research at the Rockefeller Institute for five years, then retired from active work to Nashville where he joined his only brother, Roy Avery, Professor of Bacteriology at Vanderbilt University Medical School.

Avery was richly endowed with many intellectual gifts. In his early youth he had been much interested in music and had become proficient with the trumpet. Indeed he once played in a concert given by the National Conservatory of Music of New York under the direction of Antonin Dvorak. He had also talent as a draughtsman. Several ink caricatures that he made—especially those of himself—had pictorial beauty in their simple but strong design, and revealed much psychological insight. He turned his skill later to landscape painting in water colour.

Throughout his life Avery displayed an extraordinary ability to express himself with words. While in college at Colgate he had been selected as the runner-up for the valedictorian address—his competitor was Harry E. Fosdick who eventually became one of America's most eloquent preachers. When in a playful mood in later years, Avery was wont to declaim with obvious pleasure some of the sonorous sentences on Chinese history that he had prepared for the college oration. It was a few years later during the Hoagland laboratory days that his success as a lecturer won him the appellation 'The Professor'. However, he lectured but rarely after he joined the Rockefeller Institute, but each address was a matchless performance, prepared with infinite care and delivered with much force. Each emphasis, each nuance was indicated in the manuscript of the lecture and all inflexions of voice were tried repeatedly beforehand, using the laboratory staff as a sounding board. Although reluctant to speak in public—almost coquettishly so—he was always willing and even eager to engage in conversations with either friend, collaborator or stranger. More often than not, the conversation

evolved into a monologue in which each statement, each word, indeed each hesitation, was carefully weighed and staged. The marvellous precision of the performance was not achieved by accident. Endless rehearsals assured the perfection of conversation pieces that most of us came to know by heart and referred to as 'the Red Seal Records'.

Avery was even more of a purist in the writing of his scientific publications. Each statement was the subject of much thought and discussion—as to content, form and implications. The style became rather impersonal because so laboured and polished, but it achieved by that very token a classical character of austere objectivity.

With all these gifts, it is clear that Avery could have achieved success in many different types of intellectual activity, but he made an early choice. With extraordinary singleness of purpose he devoted all his energy and talents to the problems of medical microbiology and of the pathogenesis of infectious disease.

Upon graduating from medical school Avery served for a few months as a substitute in a Park Avenue medical office. From all accounts—including his own—he was very successful in his dealings with patients but found nevertheless little pleasure in the practice of medicine. Fortunately, medical America was then becoming research conscious. Sir Almroth Wright had just been lecturing in the U.S.A. and had stimulated interest in the relation between phagocytic index and susceptibility to infection. As a result of his visit, a small research fund was established for the study of the phagocytic index in respiratory disease and this provided Avery with an opportunity to enter research in this field.

A more permanent position was offered to him a year later at the Hoagland laboratory in Brooklyn—the first institute privately endowed in the U.S.A. for bacteriological research. The director of the laboratory was Benjamin White who on his retirement was to write a lengthy monograph *The biology of the pneumococcus*, which constitutes a monument to Avery's own investigations. Alone or in association with Ben White, Avery published from the Hoagland laboratory a number of papers on several problems of bacteriology and immunity. These studies were scholarly in approach, thorough in execution, but they exhibited little originality and can be regarded as merely the products of a self-training period. One of them, however, deserves some mention as belonging to a type of systematic clinical testing, very different from the more imaginative studies that were soon to follow. Ben White, having developed tuberculosis, went to take the cure at the Trudeau Sanatorium. Avery accompanied him on the initial trip and later spent several periods of vacation at the sanatorium. This experience naturally stimulated in him an interest in tuberculosis; several of his notebooks of the time are full of extensive and carefully written analyses of the current publications on the experimental and clinical aspects of the disease. While vacationing at the Trudeau sanatorium, Avery carried out 100 consecutive

blood cultures of patients in the active phase of the disease without ever recovering tubercle bacilli or observing evidence of secondary infection. This experience demonstrated his ability to carry out a systematic clinical study. But routine work was not his bent and circumstances fortunately soon gave him the occasion to display his imaginative talents.

In 1913, Avery was appointed as bacteriologist to the Rockefeller Institute Hospital. Since lobar pneumonia was then one of the most active fields of investigation at the hospital, he joined with A. Dochez who had undertaken an investigation of the types of pneumococci present in pneumonic patients and in carriers. An important outcome of this co-operative program was the discovery of specific soluble substances of pneumococcus origin in the blood and urine during lobar pneumonia. Thus began a collaboration which persisted in an informal but highly creative form even after Dochez left the Rockefeller Institute. The two men shared a bachelor's apartment thereafter and apparently never tired of discussing night after night problems of etiology and pathogenesis. It would demand much psychological perspicacity to delineate the respective contributions of each participant in these midnight dialogues which had so much influence on the evolution of medical microbiology.

The volume and range of Avery's scientific activities increased rapidly during the following years and it would be confusing to describe them chronologically. Instead I shall outline his contributions under several categories, even though they were closely related in his mind, and were often made almost simultaneously. In this presentation, I shall omit the names of the many distinguished scientists who collaborated in the different projects. For, whatever the importance of their individual contributions, it was Avery who provided for a period of 30 years the guiding hand and the driving force of his department.

First to be considered of course are the studies on the role played by capsular polysaccharides in determining the immunological specificity and virulence of pneumococci. In brief, the findings of his department can be summarized as follows:

There exists a definite correlation between the virulence of pneumococci and their possession of a capsule detectable by microscopic and immuno-chemical techniques.

The non-capsulated variants are more rapidly killed than the capsulated forms, both *in vivo* in the normal animal, and *in vitro* in mixtures of normal serum and leucocytes. The capsule participates in virulence by increasing the resistance of the bacteria to phagocytosis.

Encapsulated pneumococci can be separated into several different serological types by virtue of chemical differences in their capsular substances. All these are polysaccharidic in nature.

Specific antibodies directed against the capsular polysaccharides protect against infection by neutralizing the ability of the capsules to interfere with phagocytosis.

Although the reactive groupings responsible for immunological specificity of the capsular polysaccharides can survive many types of chemical and enzymatic treatment, the antigenic effectiveness of the complex polysaccharide antigen is far less stable. In other words, the ability of capsulated pneumococci to elicit the production of antibodies protective against infection is optimum only when the bacteria used for immunization are prepared by techniques which maintain the antigenic integrity of the capsular material.

Immunization with pneumococci elicits the production of many antibodies directed against components of the bacterial cell other than the capsular substance (other polysaccharides, proteins, etc.). These non-capsular antibodies, however, have little if any ability to protect against infection although they may play a part in certain pathological processes, for example in those caused by allergic reactions to bacterial products.

Parallel studies carried out under Avery's influence at the Rockefeller Institute Hospital demonstrated that a similar pattern of relation of capsular polysaccharide to immunological specificity and virulence obtains also in the case of *Klebsiella pneumoniae* and *Hemophilus influenzae*. Further evidence of the relationship—if any was needed—was furnished by the demonstration that an enzyme extracted from a soil micro-organism which hydrolyzed *in vitro* the capsular polysaccharide of type III pneumococci was also capable of destroying *in vivo* the capsule of these organisms and could thereby protect mice, rabbits and monkeys against experimental infection.

The results of the studies on capsular polysaccharides soon yielded practical applications. They permitted the development of exquisitely sensitive and specific diagnostic procedures *in vitro* (serological typing) and *in vivo* (skin test for the demonstration of circulating antibody). They also guided the preparation of immunizing antigens, and of therapeutic sera, which proved their worth in practice.

Needless to say, these discoveries focused attention on the hitherto unrecognized immunological activity of polysaccharides. Fortunately, Karl Landsteiner was at that time carrying out his classical immunochemical studies at the Rockefeller Institute. His techniques were applied in Avery's laboratory to the synthesis of antigens in which polysaccharides or simple sugars were the determinant antigenic groups. A spectacular outcome of these studies was the production of a synthetic antigen which conferred to experimental animals a definite degree of protective immunity against certain pneumococcus infections.

The significance of all these discoveries can be discussed from several different points of view. For many workers, the most original contribution made by Avery was the demonstration that the role played by complex carbohydrates in various immunological processes is at least as important as that classically attributed to proteins. Of less obvious, but perhaps greater interest was the recognition that virulence and immunity can be analyzed apart from the parasite cell as a whole, in terms of some highly specialized cellular component. In the pneumococci, the important structure was found

to be the capsule, made up of polysaccharide; in other microbial species it might be some other cellular structure, with a different chemical nature. It is of interest to mention in this regard the immunochemical studies on streptococci carried out at the Rockefeller Institute Hospital. Although Avery's name appears only in the first two of these studies, his continued guidance played no small part in the recognition that a protein, the M substance, plays in the virulence of the group A streptococci a role analogous to that played by capsular polysaccharides in pneumococci. The constant theme throughout all these interests was the possibility of analyzing infection not only as an ecological phenomenon, but also in terms of the cellular components of the parasite which affect the host and against which the host reacts.

Despite these successes, Avery was acutely aware of the fact that immunological phenomena do not account for all the aspects of the pathogenesis of infection. For this reason, he was eager to investigate the metabolic characteristics of the infective agent, and the physiological-biochemical disturbances produced in the host by infection. To the former area of interest belong a number of valuable studies of the oxidative and autolytic enzymes of pneumococci, and of the growth requirements of influenza bacilli. Worthy of particular mention was the discovery that certain strains of *B. influenzae* require for growth two different vitamin-like factors (then designated as X and Vi and later identified by other workers as haem and cozymase). Commonplace as this discovery appears today, it was then (in 1921) a pioneering step in the new field of bacterial nutrition.

Avery's concern with what he was wont to call 'host chemistry' led to the recognition in the serum of pneumonic patients of a peculiar protein which appears during the acute phase of the disease and which was first detected by its ability to react with the C polysaccharide of pneumococci. It was shown that the 'C reactive protein' is not an antibody but rather a product of cellular damage released in a number of pathological processes of unrelated etiology.

In 1932 a bombshell exploded in the field of pneumococcus immunology. It was the report by F. Griffith that pneumococci could be made to change immunological specificity by certain laboratory manipulations. For many months, Avery refused to accept the validity of this claim and was inclined to regard the finding as due to inadequate experimental controls. This scepticism was understandable in one who had devoted so much effort and skill to the doctrine of immunological specificity. Avery was at that time suffering from Graves's disease and soon was compelled to leave the laboratory for some six months. When he returned in the fall of 1932 new evidence had come to light in favour of Griffith's claims; the results had been duplicated by Neufeld in Germany and at the Rockefeller Institute Hospital by M. H. Dawson. It was with reluctance that Avery eventually accepted that pneumococci could be made at will to undergo transmissible hereditary changes in immunological specificity. But once he had accepted the new phenomenon

he immediately visualized its far reaching implications not only for bacteriology and genetics, but also for general biology and medicine. He was then approaching 60 years of age and many felt that he had shot his bolt. Yet it was during this last phase of his life, and in part after his official retirement, that he made the discovery that may well prove to be his most important achievement—indeed one of the milestones of experimental biology.

He first proceeded to separate from capsulated pneumococci a soluble fraction capable of bringing about the change of type *in vitro*. Enlisting as he had so often done in the past the enthusiastic interest of younger collaborators, he soon obtained a highly purified fraction that could transfer to non-capsulated variants and to their progeny the hereditary property to produce the capsular polysaccharide of the strain used for the preparation of the extract. The climax of this study, and of his scientific career, was the demonstration that the substance responsible for the hereditary alteration of the cell was a desoxyribonucleic acid.

In summary it can be said that the investigations carried out by Avery and his school between 1913 and 1940 have provided the pattern, the master plan, used by our generation for the immunochemical study of infectious processes. His later publications, on the other hand, threw a brilliant light on problems which bid fair to constitute some of the dominant preoccupations of the workers of tomorrow; namely, the nature of those modifications of host chemistry which result from infection, and the mechanism by which hereditary characters are transmitted in micro-organisms. It was largely due to his work that microbial parasitism evolved from an ecological concept into a body of facts and doctrines which define in physicochemical terms the mechanism of host parasite relationships.

It is not surprising, therefore, that his scientific achievements as well as his wisdom soon gained him a large following in the world of science. He was truly worshipped by a host of admirers and friends. He was made president of several scientific societies even though he remained completely aloof from their social and political activities. He was recipient of the John Phillips Memorial award of the American College of Physicians in 1932, the Paul Ehrlich Gold Medal in 1932, the Copley Medal from the Royal Society of London in 1945, the Kober Medal of the Association of American Physicians in 1947 and the Passano Foundation Award in 1949. He was also the recipient of several honorary degrees: Sc.D., Colgate University, 1921; LL.D., McGill University, 1935; Sc.D., New York University, 1947; and Sc.D., Rutgers University, 1953. He was elected to the National Academy of Sciences in 1933. He was made a foreign member of the Royal Society of London in 1944. He was also an honorary member of the Société Philomathique de Paris and the Académie Royale de Médecine de Belgique. That he was not made a Nobel Laureate remains to this day a matter of painful surprise in many scientific circles, since all his discoveries had an obvious quality of perfection and finality, immediate useful application, and great influence in moulding the activities of other investigators. One might hope that the Nobel Academy

will some day acknowledge this oversight, and publicly recognize as once the Académie Française did for Molière

‘Rien ne manquait à sa gloire
Il manquait à la nôtre.’

The mere statement of Avery’s discoveries would be sufficient to secure for him an important place in the history of science. Other aspects of his scientific life are also of great interest—namely his method of work, his manner of collaborating with other scientists, the unity of spirit that he instilled among his associates despite the absolute lack of formal organization of his department. These are difficult problems to discuss in a few lines, because they involve so many judgments of values and attitudes. I shall nevertheless make bold to present a few personal views on these matters—knowing well that they will not be accepted as a true picture by others who were closely associated with Avery for many years and enjoyed his friendship.

To be blunt, it is my opinion that Avery was not as broadly informed a scholar as one could assume from his achievements and fame. He had received an excellent training in school and had read a good deal during his early days. In later years, however, he made little effort to follow modern trends in science or in other intellectual fields, but instead focused his attention on subjects directly related to the precise problem that he had under study. In the laboratory, he was limited to a rather narrow range of techniques, which he rarely changed and to which he added little. But he had a form of genius which went far beyond the pedestrian attributes of the tradesman of science. First and foremost, he had an uncanny sense of what was truly important. Among the innumerable things that could be done, he knew how to select those that were worth doing. Once he had decided on the goal, he spent countless hours—alone or in conversation—formulating and reformulating the essence of the problem in as precise terms as possible. He consulted with any one that was likely to contribute some fact relevant to the understanding of the question or some technique that might help the experimental approach. When work was under way, he was not satisfied with any but the most exacting criteria of evidence. He wanted the last experiment to be a perfect ‘protocol experiment’—one in which all the significant variables and controls had been introduced and which yielded without fail the desired result—a demonstration so obvious that it never required statistical analysis. His ideal was an experiment in which the conclusion was inescapable from the observation of a few cages of mice or a few test tubes in a single small rack. Indeed these protocol experiments were so decisive, so demonstrative, that they always had dramatic quality. This was showmanship, perhaps, but of such a high quality that it constituted an artistic performance in which all the demands of scientific integrity were satisfied.

Most experiments were carried out in close association with younger scholars, many of whom have since gained envied laurels in other fields of

research. In fact, one of the most intriguing aspects of Avery's department is the fact that such a large percentage of his former collaborators now occupy important positions in medical schools and research institutes all over the world. Few laboratories—if any—can boast of such a large percentage of highly successful alumni. It is not easy to account for the continued distinction of scientific performance by most of those once associated with Avery. This success was hardly the result of care in the selection of collaborators, for Avery never took the trouble to investigate carefully the qualifications of candidates or Fellows that joined his laboratory. Most of them came as a consequence of usual hospital appointments, or because they had been sent by some Fellowship program. There was no organized teaching or training in the department; in fact, there was no formal organization. Avery never asked or urged anyone to do anything, or to participate in any of his problems, or to initiate a new program. However, consciously or unconsciously, he had developed a subtle and effective technique to create unity of purpose among staff and visitors alike. His office was always open and he was ready at all hours of the day to welcome in his usual gracious manner the questions or statements of any one of us. But whatever the scientific problem in the mind of the intruder, Avery would soon find a way to emphasize one aspect of it that had a bearing on some problem of his own concern—as likely as not some phase of pneumococcus lore. And the conversation would thus naturally evolve into a performance of one of the 'Red Seal Records' which stated with art and precision the questions of interest to the department as a whole. Although all of us appreciated the perfection of the 'Red Seal Records', there were times when we became somewhat tired of hearing them. And yet, there is no doubt that they maintained in the department a remarkable unity of purpose. Whatever the training of the listener—clinician, bacteriologist, immunologist, chemist—his attention was soon focused upon some aspect of the departmental problems to which his particular skill was well suited. And without ever being given a task, or even being asked to participate in the work, the newcomer thus became a part of the team. More important, he himself selected the area of work best suited to his own taste and gifts.

This subtle manner of fostering co-operative action contributed greatly of course to the effectiveness and variety of the research program. Its indirect consequences were even more important, for it gave each one of us the opportunity to discover our individual attributes and to gain confidence in their exploitation. Avery did not select or train his collaborators. He created an atmosphere in which their potentialities had a chance to emerge from their unknown selves. His department was a nursery in which any form of genius could unfold.

The two views of The Professor that I attempted to convey in the opening paragraphs of this memoir were complementary aspects of a rich personality. His engaging smile, and skill in identifying himself for a moment with the

visitor, corresponded to that part of his nature which made him so perceptive and responsive with regard to the outer world. His lonely mood symbolized the phase when the phenomena and impressions that he had perceived became part of his own substance, were organized by selection and alterations into a pattern peculiar to his own personality. He was a scientist in his respect for facts and in his exacting requirements for evidence, but his scientific creation was conditioned by a highly developed artistic sense and classical taste. He despised confusion and uncertainty in facts and in language. Although his temperament was not large and robust enough to encompass the world in its discouraging if glorious complexity, he had the creative vigour to select from the confused mass of events and phenomena the few that were clearly relevant to his problems. He had the aesthetic sense to transform the apparent chaos of nature into the orderly and meaningful pattern that we call Art.

R. J. DUBOS

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H J Baker

HENRY FREDERICK BAKER

1866-1956

HENRY FREDERICK BAKER, who died in Cambridge on 17 March 1956, was at the time of his death the senior Fellow of the Royal Society, having been elected in 1898. To the present generation of mathematicians he is known chiefly as the founder of a vigorous school of geometry, but in fact his contributions to knowledge in that field, to which the second half of his life was devoted, only represent about half of his mathematical work, and the range which he covered goes far beyond the bounds of geometry. While much of his earlier work has been overtaken by the march of time, his contributions to the theory of functions, differential equations, and continuous groups had in their day as much influence as his later work on geometry, and any attempt to review Baker's contributions to mathematics must take as much account of his early work as of his later contributions.

Baker was born in Cambridge on 3 July 1866, the son of Henry Baker and his wife Sarah Anne. Little is known about his early years in Cambridge, but at the age of eleven he came under instruction from the Rev. Frederick Hatt, later headmaster of Moulton Grammar School, Spalding, who sent him in for the examinations of the Science and Art Department of the Committee in Council on Education. The examinations were on sound, light and heat, electricity and magnetism, animal physiology, physiography, geology, and mathematics. In later years, Baker referred to this period of his life more than once, attributing to the instruction he then received his lasting interest in natural science, and in particular he gratefully acknowledged the debt he owed to Mr Hatt. Except for mathematics, which was a regular school subject, the instruction he received consisted of one lecture a week on each subject. The pupils took no notes, and little practical work was done, but they were encouraged to do things for themselves. Baker in later years held the view firmly that this method did much more to foster curiosity and wonder than the laborious filling of note books. At any rate it suited him, for in each of the years 1878, 1879, and 1880 he achieved a high place in the examination, and in 1880 he was awarded a Queen's Prize. Proceeding to the Perse School, he found himself cut off from natural science, the curriculum consisting almost entirely of mathematics, Latin and Greek. He became particularly interested in Greek, and was fascinated by what he learned of the eager curiosity of the Greeks in all things natural and supernatural, their subtlety and their love of beauty, and he believed that a study of this was as good a

stimulus to a scientific life as any. In 1883 he obtained Distinction in Pure Mathematics and Greek in the Cambridge Senior Local Examinations. On the result of this examination he was awarded a sizarship at St John's, but instead of coming into residence immediately he sat for an Entrance Scholarship at that College in the following December, when he was elected to the top Foundation Scholarship.

Throughout his life, Baker maintained a keen interest in the Perse School. He was President of the Old Persean Society in 1932 and thereafter a life Vice-President.

He went into residence at St John's in the Michaelmas Term of 1884, and three years later he was bracketed with three others as Senior Wrangler. In 1888 he was placed in the first division of the first class in Part II of the Mathematical Tripos, and he was elected to a Fellowship at St John's College in November of that year. In the following year he was awarded a Smith's Prize, and in 1890 he became a College Lecturer. Apart from this bare record of achievement, there is little to record of this period of Baker's life. But during his undergraduate years he formed a lasting friendship which was to lead him to one of the great interests of his life. This was with P. J. Fagan (afterwards Sir Patrick Fagan, K.C.I.E., C.S.I.) who later was for many years Hon. Treasurer of the Fairbridge Society. Baker took a very deep interest in the work of this society, and throughout his life rendered it great service.

Baker was thus launched on his career as a college don at a time when Cayley was still active, and Forsyth was the leader of the younger mathematicians who were following in the Cayley tradition. Though Forsyth has been much criticized for his lack of rigour, he rendered Cambridge mathematics great service by his efforts to bring about closer co-operation between mathematicians in this country and those on the continent of Europe, and he made it easier for his successors, including Baker, to get the full benefit of the work of the great German masters of the late nineteenth century. In particular, it is easy to recognize the influence of Weierstrass and Klein on Baker's work. Nevertheless, there is, through all his work, a basis which derives directly from Cayley. This reveals itself in the emphasis given to algebraic manipulation. In analysis, as in geometry, while the necessity of giving a rigorous proof of the theorems under consideration is never overlooked, the sheer pleasure which Baker got from elegant though complicated algebraic manipulation is always evident, as well as his mastery of technique. This does not imply that Baker was a mere manipulator; the insight and the appreciation of the subtleties necessary to produce work of the highest quality was always there, but when this grasp of the fundamentals enabled him to achieve the main step towards the result he was seeking by the use of elegant algebra one can sense the intense pleasure which it gave him. There is an algebraic basis in nearly all of his earlier work on the theory of functions and differential equations. In later years, other influences become more apparent. This is particularly true of his later work on geometry, where, under the influence

of von Staudt and Reye, he became fascinated by the axiomatic approach to projective geometry, and he was also much influenced by the methods used by the Italian geometers, particularly Castelnuovo, Enriques, and Severi, to develop the birational theory of surfaces. Nevertheless, the attraction of algebra and the influence of Cayley never wholly left him. While he expounded these different approaches to geometry in lectures, and wrote books on them, most of his own contributions to the subject have an algebraic basis. Sometimes this is covered up in the published version by presenting the result in synthetic form (usually with a parallel algebraic treatment), but Baker once told the writer that his first attack on a geometrical problem was usually algebraic. This desire to see the algebraic significance of a geometrical theory is well illustrated in the sixth volume of his *Principles of geometry* (1933), in which he deals with the birational invariants of surfaces. He presents this subject in a form which is much more algebraic than any of the Italian writings on the subject and, indeed, his treatment has in places a marked resemblance to some of Cayley's early writings on surfaces.

For some twenty-three years, Baker led the busy life of a college don. His lectures covered a wide range, mainly on the theory of functions, differential equations, and geometry, but he was ready, when the necessity arose, to turn his hand to dynamics, electricity and magnetism, or astronomy. On one occasion he acted for a year or so as a mathematical coach, but he soon decided that this did not suit him, and he returned to the life of a college lecturer. During this period he wrote nearly all his papers on the theory of functions, differential equations, continuous groups, and matrices, as well as a substantial number of papers on algebra and geometry.

During his period as a college and university lecturer Baker displayed an astounding capacity for hard work—a capacity which never left him, and which led him both at this time and later, when he was a professor directing the work of large numbers of research students, to demand the greatest industry from his pupils. The teaching load of a college don was very heavy in those days—Baker often gave six or seven courses of lectures in an academic year, in addition to performing many other teaching duties which fell on him—and indeed it was only in the afternoons, when undergraduates were playing games, that he could find time for reading, writing books, and research. His two great books on algebraic functions, *Abel's theorem and the allied theory* (1897) and *Multiply periodic functions* (1907), which are works of great erudition and contain many original contributions which foreshadow his later work on geometry, were written under conditions which would seem incredible to many of the present generation. He never, however, thought of his industry as at all outstanding, though he did confess that in order to keep his nerves steady for his work he took to smoking at a very early age—he was always a heavy pipe smoker.

His powers were quickly recognized, and he was elected to the Royal Society in 1898, at the age of 32, and in the same year he became a University Lecturer (an office which was much more rare in those days than it is now).

In 1903, when the Cayley lectureship in the University was founded by an outside benefactor, Baker was appointed the first holder.

In 1893 he married Miss Lily Isabella Homfield Klopp, daughter of O. C. Klopp of Putney, formerly of Leer, Germany. She died in 1903, leaving two sons. Ten years later, he married Miss Muriel Irene Woodyard, of Norfolk. There is one daughter of this marriage.

In January 1914, Baker was appointed Lowndean Professor of Astronomy and Geometry in the University of Cambridge. The appointment was made by Lord Rayleigh, as Chancellor of the University, the Board of Electors having failed to agree. It is generally believed that the cause of the disagreement lay in the dual nature of the chair. The previous holder of the chair, Sir Robert Ball, had been an astronomer, as indeed, had been the five occupants of the chair before him. One party of the electors held the view that to elect a pure mathematician to the chair was, in fact, to rob astronomy; the other party considered that the two professors of astronomy (Newall and Eddington) that the University already had were sufficient. Baker's appointment by the Chancellor was no doubt hailed as a victory for geometry, but his first action as professor was to offer, in addition to the courses on pure mathematics which he had been scheduled to give as a university lecturer, a course on periodic orbits, thereby making it clear that he regarded himself as professor both of astronomy and of geometry. He continued to lecture on dynamical astronomy for many years and wrote a number of papers on astronomical subjects, and a number of distinguished astronomers have acknowledged in the warmest terms what they owe to Baker.

The outbreak of war in August 1914 made it difficult for Baker to make an immediate effect as professor, but as soon as the university resumed its normal activities after the war he set about building up a school of geometry in Cambridge, a task requiring the utmost courage, since the fashion for analysis was very strong. He quickly succeeded in gathering round him a number of enthusiastic young men, who eventually made the Baker school famous in and beyond the British Isles. In its early days, this school worked mainly on the geometry of projective space, and the journals of the twenties and early thirties are full of papers by members of the school on special configurations in space of three, four, or five dimensions. The debt which these pupils owed to Baker was immense. Though he himself contributed many important papers, his personality and his enthusiasm were probably even more important than his example. His style of lecturing was perhaps at first rather overwhelming, but one quickly caught the enthusiasm of the lecturer and shared in the general excitement which the lectures provoked. The lectures were always very carefully prepared: a configuration was first carefully explained on the blackboard, accompanied by a rapid verbal description, and then the proof was read out at a rapid rate from his notes. Most of us soon learned how to take down our notes, but, fortunately for those who found the pace too great, a great deal of the subject matter of his lectures of this period eventually appeared in the first four volumes of his *Principles of*

geometry (1922-25). Later he turned his attention more to the birational theory of surfaces, which the Italian school of geometers had been developing from about 1893. His interest in this topic was indeed of long standing, and had been the subject of his Presidential Address to the London Mathematical Society in 1911. This linked up with his earlier interests in the theory of algebraic functions, and although his later lectures on birational geometry followed the Italian geometrical methods, when he came to write the fifth and sixth volumes of the *Principles*, which dealt with birational geometry, he gave, in the fifth volume, an account of the function-theoretic aspects of the theory of curves which contained many additions of his own.

No account of Baker's school of geometry can be complete without a mention of his 'tea-party' which he held each Saturday afternoon in term time during his tenure of his professorship. This was the prototype of the numerous seminars which are held weekly nowadays, but was for a long time the only one of its kind. Although there was always an atmosphere of great friendliness, nevertheless it seems to the writer that the protocol was more strict than appears to be the case nowadays. The party, sometimes numbering fifteen to twenty, and having in common only their devotion to geometry, met at 4.15 p.m. for tea, and all were expected to attend—a 'blue' who combined athletic prowess with an interest in geometry was allowed to come late, but he was expected to put in an appearance as soon as he could. After tea one member of the party gave a talk on a geometrical topic, and then the matter was open to discussion. The audience had to keep on its toes, for if there was any pause in the discussion Baker was liable to call on anyone for an opinion. At the height of its popularity there was no difficulty in finding speakers, as there were always two or three young men ready with new results that they wished to describe, but in early days, when numbers were smaller, any member of the party might be detailed to get up a paper and read it the following week. For some newcomers it was quite an ordeal to address the tea-party for the first time, for in the discussion that followed no quarter was given, but, though Baker was not prepared to tolerate any nonsense, he was always ready to shelter the innocent speaker when his contemporaries became too fierce. Through these tea-parties one soon learned to look on Baker not merely with admiration, but with genuine affection, and to this much of his success was due.

Baker retired in 1936. Though he was seventy he was not ready to lead a life of leisure. For a considerable period after his retirement he continued to publish papers on geometry; indeed, his last appeared when he was 85. He also wrote two more books: *An introduction to plane geometry* (1943), whose aim was to give an account of the geometry of conics by the methods which he had made peculiarly his own; and a Cambridge Tract (1946) dealing with a configuration associated with a particular group of linear transformations which had always fascinated him. And only a few months before his death he completed a volume of *Mathematical gleanings* which dealt with the same subject as his *Introduction*. Most of all, he delighted to keep in touch with his

old pupils during his retirement, and any success they achieved gave him great joy.

This sketch of Baker's life suggests that it was entirely spent in the intimate atmosphere of a Cambridge which has now greatly changed. And so, to a large extent, it was. But Baker had many contacts with foreign mathematicians with whom he maintained friendly correspondence. In his earlier years he paid visits to Felix Klein in Göttingen, and this had a great influence on his subsequent work, and, as Treasurer of the meeting of the International Congress of Mathematicians in Cambridge in 1912, he made the acquaintance of many of the leading mathematicians of the day, and established friendships which he was glad to renew on the occasion of at least one later International Congress. His essential humility tended to make him think of these leaders of mathematical thought as belonging to an aristocracy to which he did not belong, and it has sometimes seemed that if he had been less modest and more ready to strike out a line for himself, instead of expounding and elucidating the work of others, his fame might have been greater.

He received his share of honours. The London Mathematical Society, of which he was President in 1910 and 1911, awarded him the de Morgan Medal in 1905, and the Royal Society awarded him the Sylvester Medal in 1910. In 1923 Edinburgh University bestowed on him the honorary degree of LL.D., and in 1943, he was elected an Honorary Fellow of the Royal Society of Edinburgh. He was President of Section A of the British Association in 1913.

Baker's active mathematical life falls into two contrasting parts, which are roughly equal both in the time occupied by them and in the extent of his writings in them, together with a short transitional period which may be said to have extended from 1911 to 1914. In the earlier period he belonged to the school of Cayley and Sylvester; this does not mean that he was just an imitator or that his ideas remained static; there is a continuous development to be marked in his writings, but there was no violent change of attitude corresponding to the change brought about in Cambridge mathematical analysis by G. H. Hardy in the early years of this century.

The most obvious difference between Cambridge mathematics before the early years of the present century and mathematics between the two world wars was that in the earlier period mathematicians did not specialize to the extent they did later, and they took the whole of mathematics as their field; whereas between the wars mathematics became so specialized that intercourse between mathematicians in the same university became almost impossible. It is true that, except for his later ventures into dynamical astronomy, Baker's writings deal entirely with pure mathematics, and that of his papers written before 1914 by far the largest group deals with algebraic functions and multiply periodic functions, but this does not indicate specialization in this field, only that Baker felt he had most to say on these topics. All through this first period he continued to write papers on other branches of pure mathematics, and these never give the impression that Baker was con-

sidering changing his field of interest or was 'having a holiday'. These other subjects were as much his concern as algebraic functions, and when he read any paper which stimulated his imagination he felt urged to put down his thoughts on it and to make his own contribution.

In recent years the tendency towards specialization has been reversed, and mathematicians are again inclined to embrace their subject more widely. Yet the contrast between Baker's first period and modern mathematical writing is as sharp as ever. This is probably due to the pace of modern life, and the compulsion which all modern mathematicians feel to keep producing something new. When Baker began writing the progress of mathematics was more leisurely, and it was possible—particularly for a man with Baker's stupendous capacity for hard work—to know all, or nearly all, that was going on in the mathematical world, and the need to push outwards the frontiers of knowledge was not the over-riding consideration which it is nowadays. The desire to understand what was written by others, to examine it from all angles, and then to expound it in such a way that fresh light was thrown on it, came first. This involved a great deal of original work of high quality; but it also meant a good deal of repetition and explanation of the work of others. Many of Baker's most important papers are of this character, and those who will take the time to study them will be well rewarded by the illumination they will derive. In assessing Baker's contributions one must always bear in mind how conditions have changed since many of these were written.

Various factors contributed to the change in Baker's interests which led to his emergence as a geometer pure and simple. The primary factor was, of course, the natural development of his ideas. His work on algebraic functions had always made clear his interest in the geometrical aspects of the subject. This was in the main in the field of birational geometry, but, later, his researches into multiply periodic functions led him to investigate the quartic surfaces of Weddle and Kummer at considerable length, and his interest in Reye's *Geometrie der Lage* caused him to consider these configurations from the point of view of pure geometry. Finally, when his researches led him to consider involutions of sets of points in a plane, and with his discovery of Castelnuovo's remarkable proof that such involutions are rational, he was introduced to the vast theory of surfaces created by the Italian geometers. This impressed him so much that he made his Presidential Address to the London Mathematical Society in 1911 the occasion for a comprehensive summary of the theory of surfaces; this summary was so able that in all bibliographies of the theory of surfaces it is quoted as one of the standard general accounts. Baker's increasing preoccupation with geometry made him the obvious candidate for the Lowndean professorship if a pure mathematician was to be appointed, and when he was appointed in 1914 it was inevitable that his highly developed sense of duty would lead him to concentrate more and more on geometry. When normal academic life was resumed after the interruptions of the war Baker assumed his position unequivocally as a geometer.

There was, however, another important factor which played a part in Baker's emergence as a geometer. This was the upheaval in Cambridge mathematics brought about by the emergence of the analytical school under Hardy. This revolutionized mathematics at Cambridge; on the one hand it led to the reform of the mathematical tripos (including the end of the order of merit and the Senior Wrangler) in an effort to modernize the teaching, and, at the research level, it led to an intense concentration on the foundations of analysis and on mathematical 'rigour'. There is no direct evidence that Baker took a leading part in the great conflict of 1906 which raged round the proposed reform of the tripos, but he certainly voted with the reformers in the Senate House. But, on the other hand, he did not modify his approach to mathematical research under the influence of the new school. It is always difficult for a mathematician in his forties to make a sudden change in his way of thinking, and in any case it is doubtful if Baker was in full sympathy with the new ideas. He believed, of course, in the necessity of laying proper foundations in any branch of mathematics—as his later writings on the foundations of geometry show—and insisted that a proof must indeed be a proper proof, but at the same time he never felt that the foundations of mathematics was part of mathematics, as the following quotation from the preface to volume 5 of his *Principles of geometry* makes clear. 'The study of the fundamental notions of geometry is not itself geometry; this is more an Art than a Science, and requires the constant play of an agile imagination, and a delight in exploring the relations of geometrical figures; only so do the exact ideas find their value.' But the new influences at work in Cambridge meant that fewer students showed an interest in the kind of function theory in which he excelled, and since there was a wide field in geometry where he could be of great influence he turned to this naturally and hence the advent of new analysis no doubt helped to turn Baker to geometry.

In his work on geometry, Baker showed many of the same qualities which had distinguished his work on function theory. While he was capable of originality of thought, his contributions seem to be inspired more by active curiosity than by inventiveness. Many of his papers are clearly inspired by the work of others, and in these he frequently points out things missed by the original writer. Another aim which he had constantly in mind was to bring order into a subject which, by its very nature, was inclined to be rambling. The first four volumes of his *Principles of geometry* represent his efforts to reduce projective geometry to some semblance of order. But above all he was concerned to excite enthusiasm amongst his many pupils, so that the subject which he loved above all others would be carried on by those who came after him. No better words can be found to describe how Baker discharged his duty to mathematics than by applying to him the words which he wrote in the *Journal of the London Mathematical Society* about Felix Klein on his death in 1923: '... Original as he undoubtedly was to a very high degree, and anxious that his view should prevail if possible, his actuating impulse, in his later years, was not so much a desire for discovery as a desire

to share his thoughts with others. Intensely sympathetic, capable of great enthusiasm about the results of others . . . , he seems to have done his thinking by conferences and lectures, and . . . to have accepted it as a matter of course that many of his results would have been obtained by others before him. He could not be like Sylvester's oyster who, in darkness and obscurity, elaborates the pearl to deck the princess. Thus he must be judged by the influence he exerted rather than by the new results he found. This influence was enormous and, among those who are wise enough to study his papers, will remain for many years to come.'

Against this background we may consider briefly some of Baker's main contributions to mathematics. Commencing his post-graduate work under the guidance of Cayley, his first contributions dealt with the algebraic theory of invariants. A few short notes preceded his first important paper (1889), which is concerned with the enumeration of the concomitants of three ternary quadrics. (The results had indeed been forestalled in Italy, but he was unaware of this.) The methods used here are clearly derived from Cayley, but at the same time the treatment has all the characteristics which are to be found in his subsequent work; the need to investigate a problem from different points of view in order to obtain the maximum light upon it, and the care taken to explain the relation between different modes of treatment are there. Some of the techniques employed in this paper were used subsequently in many other papers, but he only wrote a few more papers on invariant theory in the course of his life, and these are more geometrical than his early papers.

Following his initial series of papers, there was a relatively quiet period which, however, included two visits to Göttingen, where he came much under the influence of Felix Klein. When he again ventured into print it was clear that he had found a subject which had captured his imagination. During the next five or six years his writing was nearly all on the theory of algebraic functions. During this period he was busy reading the work of all the great masters of this subject—Kronecker, Dedekind and Weber, Weierstrass and so on, and he became the recognized authority on algebraic functions. The result of a period of the most intensive work appeared in 1897 in his first book, *Abel's theorem and the allied theory, including the theta functions*. In this many of his previous contributions were incorporated, but there is one early paper (1893) which deserves special mention, for it illustrates so well Baker's way of thinking. A great deal of the theory of algebraic functions can be described as general theory, and many an expert is content to leave it at that. He would be hard put to it to compute the genus of a given equation, or to write down an integral of the second kind, having an assigned singularity on a Riemann surface. This kind of thing was anathema to Baker; he would ask what was the point of knowing all about the subject if you could not do these simple things. The paper referred to is the result of Baker's putting these questions to himself while he was mastering the subject, and it gives a highly ingenious method of computing the genus of an equation by using the Newton polygon. Needless to say, the method is exploited with characteristic thoroughness.

Abel's theorem is a monumental work of some 700 quarto pages, and was written for those who already knew the elementary theory of algebraic functions, and who wanted to learn about the more advanced developments; so the topological properties of the Riemann surface, and the elements of the theory of algebraic integrals are assumed. The subject matter concerns the various classes of functions which can be considered on a closed Riemann surface, and the emphasis is on the way in which these can be manipulated and how functions of special interest or importance can be constructed from more elementary functions. Elementary properties of functions of a complex variable, such as the theory of residues, are of course used, but the main tool is skilful algebraic computation. It is significant that Riemann's method of proving the existence of everywhere finite integrals is not used, and it seems probable that was due to Baker's feeling that a general existence theorem which could not be applied to a particular case was not satisfying, rather than to the fact that the method was temporarily under a cloud owing to a gap in Riemann's proof.

As with most of Baker's work, it is not easy to isolate the author's original contributions from the work of others. Very detailed accounts of the work of Kronecker, Dedekind and Weber, and many others are given, but the presentation is fresh, and much has been added in the course of writing, both with a view to carrying the work further and to reconciling different viewpoints. And again, the geometrical interpretations of many of the results, which play an important role, probably contain many new results, but here again no clear distinction is drawn between what is old and what is new. It is, indeed, typical of Baker that he so writes down his own contributions that they are apt to pass unnoticed. But what cannot be hidden is the fact that he welded a mass of diverse material into a single whole of great beauty.

The book, as its title proclaims, includes an account of the theta functions, and indeed it has something to say on the general theory of multiply periodic functions, but this part of the work is kept within the bounds set by the general purpose of the volume. But it forms the starting point of what may be regarded as the sequel. During the next ten years Baker wrote many of his important papers on multiply periodic functions, and associated functions such as hyperelliptic functions, sigma functions and Jacobian functions. These include some of the most important contributions of his pre-1914 period, and once again many of his results were eventually incorporated in a book, his *Multiply periodic functions* (1907). Many of his researches arose from his wish to extend to several complex variables the beautiful and complete theory of elliptic functions. Of these his papers of 1897 and 1902 were probably the best. In the first of these he sought to generalize the formula

$$\frac{\sigma(u+v) \sigma(u-v)}{\sigma^2(u) \sigma^2(v)} = \frac{d^2}{du^2} \log \sigma(u) - \frac{d^2}{dv^2} \log \sigma(v),$$

obtained by Frobenius for sigma functions of one variable, to functions of

several variables; as is well known this formula plays a fundamental role in the theory of elliptic functions; Baker succeeded in extending this to the hyperelliptic case, so being able to construct his sigma functions.

Another problem which was the subject of several of Baker's papers was the determination of the number of common zeros of n multiply periodic functions of n variables. In the case of theta functions, Baker seems to have disapproved of the degeneration method used by Poincaré, and he made strenuous efforts to avoid it. His method was to determine a curve on which the variables could be treated as integrals of the first kind forming a defective system and to solve the problem by means of the Riemann surface of the curve. Many of his results in this field are included in his *Multiply periodic functions*. The first part of the book is an account of the theory of hyperelliptic functions of two variables, and includes a detailed study of the geometry of the Weddle and Kummer surfaces, and the second part is devoted to multiply periodic functions, particularly the problem of the zeros of a set of Jacobian functions.

Much of the work done by Baker in the theory of algebraic and multiply periodic functions remains where he left it; though there has been increasing interest in these subjects in recent years, the point of view has been very different. But some of Baker's incursions into other fields have a certain topical interest, and we must now refer to one or two of these. Mention should first be made of his contribution to the theory of differential equations, in which he made extensive use of the theory of matrices.

Baker considered a set of r simultaneous linear differential equations of the first order in r unknown functions $y^1, \dots, y^r$, which can be written in matrix notation

$$\frac{dy}{dx} = \mathbf{u}y$$

where $\mathbf{u}$ is an $r \times r$ matrix of functions of the independent variable x . In most cases of interest the elements of $\mathbf{u}$ are analytic functions of the complex variable x except at a finite number of places. The Frobenius method of solving the equation by series has the disadvantage that in general the series only converges in a small region; Baker was concerned to obtain a solution valid in any connected domain contained in the region obtained by drawing cuts from the singularities to infinity. If z_0 is any fixed point of the domain and z a variable point, denote by $Q(\mathbf{u})$ the matrix obtained by integrating the elements of the $r \times r$ matrix $\mathbf{u}$ from z_0 to z along a path in the domain, and let $Q_r(\mathbf{u})$ be defined by the equation

$$Q_r(\mathbf{u}) = Q[\mathbf{u} \, Q_{r-1}(\mathbf{u})]$$

Baker proved the convergence of the series $\Omega(\mathbf{u}) = \mathbf{1} + \sum_1^{\infty} Q_r(\mathbf{u})$ and

proved that if $\mathbf{A}$ is any constant column vector then $\mathbf{y} = \Omega(\mathbf{u}) \mathbf{A}$ is the most general solution of the equation. By replacing a linear differential equation

of order r by a set of r simultaneous equations of first order, he was able to obtain the solutions of equations of the form

$$a_0(z) \frac{d^r y}{dz^r} + a_1(z) \frac{d^{r-1} y}{dz^{r-1}} + \dots + a_r(z) y = 0,$$

valid in any simply connected domain of the complex plane which does not contain a singularity of the coefficients. (This method of solution is also associated with the name of Peano.)

Baker made many applications of this method. His 1916 paper, in which he applies his methods to certain differential equations which arise in astronomical problems, calls for particular mention. In these equations $r = 2$, and the coefficients are periodic functions of x with period 2π . The solutions are (in general) of the form $e^{\lambda x} \phi(x)$, $e^{\mu x} \psi(x)$, where λ , μ are constants and $\phi(x)$ and $\psi(x)$ are periodic with period 2π . Baker investigated the values of λ , μ particularly in respect of the reality of these values. He also considered the equation

$$\frac{d^2 y}{dx^2} + y(1 + 4a \cos hx + 4b \cos kx) = 0,$$

where a and b are small constants, which Poincaré had investigated. The equation has periodic solutions when h and k are commensurable, and Poincaré claimed that the convergence of series expressing the solution depended on this circumstance, but Baker showed that his solution converged as a series in a and b without any restriction on h/k .

Baker also made use of matrices in connexion with the theory of continuous groups in a number of papers which still have an important place in the subject. (He was particularly pleased when fifty years later he was asked for reprints of his papers on this subject by some leading American mathematicians.) His work in this field dealt with the so-called exponential theorem for a simply transitive Lie group of dimension r . Let $C_{\alpha\beta}^\gamma$ be the constants of structure of the group, and let (e^α) , (f^α) , (g^α) be sets of canonical parameters such that (g^α) are the parameters of the operation obtained by following the operation (e^α) by the operation (f^α) . If $E(e)$ is the $r \times r$ matrix

$(C_{\beta\gamma}^\alpha e^\gamma)$, and $\Delta(e) = 1 + E(e) + \frac{1}{2!} E(e)^2 + \dots$ is the exponential of the

matrix $E(e)$, then it is shown that

$$\Delta(g) = \Delta(f) \Delta(e).$$

Baker then shows that the equation can be solved to give the finite equations of the group and the consequences of this are fully exploited. The basic result on which these results are founded is obtained by pure computation, but the underlying algebra intrigued Baker so much that he subsequently wrote another paper (1905) to elucidate this.

Another group of papers which is of some current interest deals with the general theory of functions of several complex variables (1900, 1902, 1903). Weierstrass had suggested the problem of showing that the theorem that a meromorphic function of one complex variable could be written as the quotient of two holomorphic functions could be extended to meromorphic functions of several variables, and Poincaré had considered the problem of showing that a non-singular hypersurface in complex affine n -space given locally by expressing the co-ordinates as convergent power series in $(n-1)$ variables could be given globally by the vanishing of a single holomorphic function. Poincaré's solution depended on constructing the potential function for a conductor occupying the position of the locus of zeros. Baker's solution (1900) was an improvement of this which brought in the imaginary part of the function simultaneously with the real part. The solution is therefore obtained as a $(2n-2)$ -fold integral over the locus of zeros. In the 1903 paper Baker used a somewhat similar method to solve the Weierstrass problem. These solutions are of great interest when contrasted with the solutions now obtainable by the modern techniques available.

Turning now to Baker's work in the years following his election to the Lowndean professorship, it has already been mentioned that as astronomy was mentioned in the title of his chair he considered it his duty to lecture on astronomy. But he not only lectured on this subject; he engaged in research on it, and wrote a number of papers on dynamical astronomy, in the early post-war years. While he eventually gave this up, he retained his interest in it and even after his retirement he corresponded with astronomers on matters of scientific interest.

We have already referred to Baker's contribution to our knowledge of the differential equations which arise in connexion with certain astronomical problems. Mention should also be made of another significant contribution to dynamical astronomy in a paper (1920, see also 1925) on rotating liquids. In this he considered a criticism which Jeans had made of some results of G. H. Darwin, and also gave an account of the work of Liapounoff which had previously not been understood or appreciated by British mathematicians.

We now come to a consideration of Baker's contribution to geometry. As we have already seen, he had always been much concerned with the geometrical aspects of his work on algebraic functions, and his study of multiply periodic functions led him to a detailed investigation of the geometrical properties of the Weddle and Kummer surfaces. But the first clear indication that he was turning to geometry for its own sake was his proof of the double-six theorem in 1911, and this was followed by his Presidential Address to the London Mathematical Society (1913) in which he summarized, in considerable detail, the work of the French and Italian mathematicians on the birational invariants of surfaces and the associated theory of algebraic integrals.

In 1911 and 1913 he also wrote two papers on cubic surfaces which set the course for much of his subsequent work. The starting point of the former is

the theory of the bitangents of a plane quartic curve, and much of the paper indeed is concerned with these. But the relation between the bitangents and the lines of a cubic surface is soon brought in and the relation fully exploited; later, in his lectures, Baker tended to reverse the process, regarding the cubic surface as the natural starting point, and deducing properties of the bitangents of a quartic from it. The paper also contains a great deal of information about the configuration of lines on a cubic surface, and the associated ways of writing its equation. The second paper uses the cubic surface as a simple means of illustrating Severi's theory of the base for curves on a surface.

From 1918 until 1951 Baker wrote about 35 papers on geometrical subjects, the majority on projective geometry, but a considerable number also dealt with birational geometry and the foundations of projective geometry. Many of these were mere notes, and seem to have arisen out of discussions with his students, and a number are commentaries on papers by others. It is not necessary to review them all, and reference to a few of the more important will suffice.

Much of the research work of Baker's school in the twenties and early thirties dealt with general properties of loci in projective space. To this work Baker himself made a number of noteworthy contributions. Mention may be made of his paper of 1932 in which he considers the enumerative problem of determining a large number of the projective invariants of a surface in four dimensions whose only singularities are improper double points, in terms of the four basic characters—the order, the genus of a hyperplane section, the arithmetic genus and the Zeuthen-Segre invariant. Amongst the invariants calculated are the order and genus of the intersection of the surface with the cone formed by the chords through a point, and the order and genus of the intersection of the surface with the cone formed by the trisecants through a point of the surface.

Baker once confessed to the writer that his first attempts to investigate a problem usually involved the use of very uncouth methods; but he took infinite pains to polish his results before publication, often succeeding in the process in isolating the essentials from a mass of detail previously assumed necessary. A good example of this is to be found in his paper (1936) on the nodes of a Segre cubic primal in space of four dimensions. Let $(x_1, \dots, x_5)$ be co-ordinates in space of four dimensions, and let (i, j) denote the point obtained by putting $x_i = 1 = x_j$ and the remaining co-ordinates equal to -1 . The ten points (i, j) are the nodes of the cubic hypersurface

$$x_1^3 + x_2^3 + x_3^3 + x_4^3 + x_5^3 - (x_1 + x_2 + x_3 + x_4 + x_5)^3 = 0,$$

and these ten points can be divided into two sets of five which are vertices of simplexes which are polars of one another with respect to the quadric

$$x_1^2 + x_2^2 + x_3^2 + x_4^2 + x_5^2 - (x_1 + x_2 + x_3 + x_4 + x_5)^2 = 0,$$

and this in six ways. The pairs of simplexes may be determined as follows. Let 1, 2, 3, 4, 5 represent five points in a plane; these can be taken as the

vertices of a pentagon in 12 different ways if the direction in which we go round the pentagon is ignored. Corresponding to each pentagon there is a diagonal pentagon and the relation between a pentagon and its diagonal is symmetrical, so that we get a pairing of pentagons. Let (i, j, k, l, m) be any derangement of $(1, 2, 3, 4, 5)$. Corresponding to it there is a pentagon whose sides are $(i, j), (j, k), (k, l), (l, m), (m, i)$ and the sides of the diagonal are $(k, m), (l, i), (m, j), (i, k), (j, l)$. The points in four dimensions represented by these two sets of symbols are vertices of two polar simplexes. From these simple observations Baker is able to deduce all the properties of the elaborate configuration associated with the nodes of the Segre primal. As a final example of Baker's contributions to geometry we may consider his paper (1937) in which he considers an involution of sets of m points on a rational normal curve of order r ($r < m$). The problem is to determine the locus described by the hyperplanes which contain r points belonging to a set of the involution. Various cases had been considered by Meyer, White, and others; Baker tackles the general problem by lifting the whole problem into space of m dimensions. It is an excellent example of the way in which Baker pondered a configuration until he could see how to attack it in its full generality. Another example of Baker's gift for adding to the work of others is his 1926 paper on 2-2 correspondences.

These remarks serve only to illustrate the range of Baker's interests in geometry, and the way in which his own investigations went hand in hand with those of his pupils, sometimes with Baker leading, sometimes with the pupils leading and Baker following with a commentary. But, essentially, Baker was a teacher, and his researches in geometry were ancillary to his lectures which in turn were published in his six volumes on the *Principles of geometry*. It is in these volumes that one really learns to understand Baker's contribution to geometry. There is so much detail in these that it is impossible to give the reader any real idea of what is in them, and the few remarks that follow can serve only as a brief indication.

The first volume deals with the foundations of projective geometry. To most of his readers, this formal laying of the foundations by axiomatic methods was new and exciting, though in fact to those acquainted with the writings of European and American mathematicians on the subject it was a little disappointing. The weakness was probably due to temperament; normally foundations in mathematics are developed step by step from axioms, with no digressions; but Baker preferred to exhibit alternative methods of approach without reconciling them completely. Baker's idea seems to have been to talk about the foundations rather than to build them up in an orderly fashion. As has already been remarked, he did not really consider the foundations of geometry as part of geometry, though he recognized the importance of laying foundations. Indeed, he does not seem to have been entirely satisfied with this part of his work, for in his last years he returned to the subject with some later reflections, notably in two papers written in 1951.

In his next two volumes he was on much firmer ground. These two volumes

deal with the projective geometry of space of two and three dimensions, and did more than anything else to establish the Baker school. To anyone fresh from school, the approach was novel, and owing to the very compressed style not everyone could take to them; but those who did became enthusiastic and were ready recruits to the Baker school of geometry. Baker was much concerned to present his ideas in as elementary form as possible, and later wrote his *Introduction to plane geometry* (1943) which he hoped would appeal to sixth form boys as well as undergraduates, but in fact it has never succeeded in its purpose, though it forms a valuable companion to the second volume of the *Principles*.

The fourth volume of the *Principles* was a fitting climax to the work which had gone into the earlier ones. These had had the instruction of undergraduates as their aim; the fourth volume was written for mature geometers and dealt with the principal configurations of space of four and five dimensions; and in particular it brought out brilliantly the idea of placing each configuration in its proper setting; for instance, the ordinary Pascal configuration associated with six points on a conic is shown to be greatly simplified by lifting it up onto a nodal cubic surface, and finally is exhibited in terms of a simple configuration in space of four dimensions. The main criticism which can be levelled at this volume is that the subject really demanded more space than Baker gave to it; what is in it is too condensed, and there is much that might have been added had Baker not confined himself too rigidly to the pattern of the earlier volumes.

Volumes V and VI, which followed after a considerable interval, do not really belong to the plan of the earlier volumes. Indeed, their existence is in flat contradiction to the intention of the author as described in the preface to volume I, and it is to be regretted that Baker, unnecessarily, confined them to the pattern of the earlier volumes. Nevertheless they are of considerable interest. They owe their origin to the fact that in the years immediately preceding his retirement Baker was mainly interested in the work of Italian geometers on birational geometry. Volume V deals with the birational geometry of curves, and the associated transcendental theory. Much of this links up with his earlier work on algebraic functions, but there are many new points of view, and the volume is, for its size, the best account known to the writer of the various ways of treating the subject.

Volume VI represents an account of the work done by Baker and his pupils on the theory of surfaces. The main topic is the determination of the principal invariants of surfaces under birational transformation, and much of the effort goes into determining the effect of singular points on these invariants. Only the simplest kind of singular point can be considered, and much of the work goes to show the essential difficulty in defining the invariants when singularities are present. The volume helped a good deal to clear up certain obscurities, but so much has been done since it was written (much more than in the fields covered by the earlier volumes) that it now seems somewhat old-fashioned.

Although not part of the *Principles*, mention should be made here of Baker's Cambridge Tract (1946). This little book, written when the author was aged 80, deals with the geometry associated with a certain linear group which has many interesting properties. The treatment, by geometrical means, throws great light on the group; but more important, it gave rise to considerable activity on the part of his pupils, and of pupils of pupils. To see his work carried on by his successors is always a pleasure to a mathematician; to be able to provide the starting point of a series of noteworthy papers when in his eighties is given to few. To Baker, who was so anxious that his ideas on geometry would survive, it was a supreme joy.

W. V. D. HODGE

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P. A. Buxton.

PATRICK ALFRED BUXTON

1892-1955

PATRICK ALFRED BUXTON was born in Hyde Park Street, Paddington, on 24 March 1892, and died at his home in Gerrards Cross, Bucks on 13 December 1955. His father was Alfred Fowell Buxton, a busy banker and one time chairman of the London County Council, his mother was Violet Jex-Blake.

His father's family was part of that immense group of Buxtons, Gurneys, Barclays, Hammonds, Hoares, many of them resident in Norfolk and many of them Quakers, whose names appear in the annals of business in this country, in banking, in brewing, in the wool trade and so on, over many centuries. This reputation for acumen in business was often combined with a most practical interest in philanthropy and in political or social reform. One may recall the active part played by Sir Thomas Fowell Buxton in association with Wilberforce in the abolition of the slave trade; and the part played by his sister-in-law Elizabeth Fry in prison reform, and of his grandson Sydney Charles Buxton who sponsored the Wild Birds Protection Act of 1902 and did much to improve conditions of employment. Some branches of the group, for example Hanbury the gardener and Francis Galton, were prominent in science—but to trace a connexion between Patrick Buxton and Galton one would have to go back five or six generations to David Barclay, son of Robert Barclay 'the Quaker apologist'. Among the distinctions of these ramifying families literature is hardly represented and the arts not at all—most were country gentlemen, good naturalists, and strongly averse to things academic.

His mother's family was not numerous; they were mostly farmers and landowners in West Norfolk. His grandfather on his mother's side, Thomas William Jex-Blake, was a classical scholar, Headmaster of Cheltenham and Rugby schools and then dean of Wells—with a strong antipathy to biology and the sciences in general. He married a Cordery and had a large family, several of them very able, among whom one might mention Kathleen Jex-Blake, classical scholar and Mistress of Girton, Cambridge, Henrietta Jex-Blake, Principal of Lady Margaret Hall, Oxford and Arthur Jex-Blake an able scholarly medical man.

There were three children in the family of Alfred Fowell Buxton, of whom Patrick was the eldest. His brother Denis Alfred Jex Buxton (*b.* 1895), also a keen naturalist, followed a career in the City; his sister Violet Elizabeth (*b.* 1900) remained at home and died a few years ago. Until Patrick was eight years old the family lived in London—in the days when it was still

possible for a small boy to ride his pony from the Edgware Road across to the Marble Arch. But there were long summer holidays in the countryside of Hertfordshire. The family then moved to completely rural surroundings, several miles from the railway, at Chigwell in Essex.

Outside his busy life as a banker, Alfred Fowell Buxton was fond of gardens, shooting, birds and moths and this was a strong influence on the small boy. There was a family tradition of knowing plants, animals and birds and of collecting them. The young naturalists were most industrious, preserving and labelling, and 'making collections'; there was a family *Natural History Journal* (an old Quaker custom) and at rare intervals they would record the catching of a water shrew, seeing a rare bird or finding a flower at an unusual date—but no one guided them further to think about their specimens or use them.

Patrick Buxton was educated at home until the age of ten. By that time he had already started on Greek—and had been imbued with a strong and lasting prejudice against the classical languages. This was followed by three years at a preparatory school with more classics and little profit, and from 1905 to 1911 he was at Rugby. Under the strong Jex-Blake influence he was kept on the classical side, which bored him to the extent that he was nearly superannuated. Indeed, looked at on the surface his school days were not a success. Short sighted, untidy in appearance, a near failure at work and with no aptitude for games, he became something of a butt—the sort of boy whose subsequent distinction his school fellows find hard to credit.

There was one bright spot. This strange youth was allowed to neglect organized games for the pursuit of natural history; he ran the school *Natural History Society* and published many local observations in their annual report. Of formal teaching of biology there was none, and scholarships and prizes did not come Patrick Buxton's way.

In October 1911, again in accordance with a strong family tradition, he went up to Trinity College, Cambridge. His tutor was the physiologist Walter Fletcher to whom he explained that he was a complete failure at academic work and proposed to take a Pass Degree in biological sciences. But Fletcher insisted that he should at least try for a Tripos and that if that proved impossible they could think about the Pass Degree after a year.

That was an encouraging start; for Buxton quickly discovered that he had been completely misled about his abilities—that he could actually do intellectual work in biology as well as the next man; and in 1914 he obtained a 1st class in Part I of the Natural Sciences Tripos with zoology, botany and physiology as his subjects.

About November 1913 Buxton had added medicine to his burdens—under a philanthropic urge that this was of more use to humanity than pure biology. And on the outbreak of war in August 1914 he enlisted as a private in a Field Ambulance, thinking that a medical student should be of some use in such a place. But in January of the following year he was sent back to his medical studies.

In the summer of 1915 he rather rashly sat for Part II of the Natural Sciences Tripos in zoology, for he had been mainly working on his anatomy and physiology for the 2nd M.B. examination, and it gives some indication of the enthusiasm with which Buxton was developing his newly discovered powers, that he not only passed the 2nd M.B. but obtained a 1st class in the Tripos.

In the old Department of Zoology at Cambridge in those days, conditions were so informal that the keen undergraduate could sit about and pick up all the methods that he wanted and learn informally from men a little older than himself in a way that is less easy in these crowded days. In physiology Buxton owed much to his informal contacts in this way with Keith Lucas, in the days when he and A. V. Hill were doing such brilliant work in such unsavoury surroundings.

In the autumn of 1916 Buxton was elected to a Fellowship at Trinity. This was awarded mainly for a piece of undergraduate research on the brain of the primitive moth *Micropteryx*. The idea of this research was entirely his own and was not very logical: the object was to attempt to throw light on the structure of the insect brain by studying the brain in a primitive insect—but *Micropteryx*, though a primitive Lepidopteron, is a member of one of the more recently evolved groups of insects. It must have been difficult work to assess, remarkable chiefly for the extremely difficult conditions, in camp and so on, under which it was carried out; but Stanley Gardiner, who was one of the assessors, and who must have started without any knowledge of the subject, doubtless recognized the potentialities of the author.

Buxton proceeded to St George's Hospital and qualified in medicine (M.R.C.S., L.R.C.P.) in 1917. He at once took up a commission in the Royal Army Medical Corps, and was posted to Mesopotamia and North West Persia. Often there was very little to do and, in consequence, plenty of opportunity for the enterprising naturalist to collect great numbers of insects, bird skins and the like. The censor raised no objection to the provision of adequate data and most of the labelled specimens reached home safely. All this material, with much else obtained by other collectors, formed the basis of a survey of the fauna of Iraq published by the Bombay Natural History Society. It also provided a fine training in the natural history of desert animals, although, being constantly on the move, it was impossible to do any serious biology. This lasted until the end of 1919.

During 1920 and 1921 Buxton was back in England, resident mostly in Cambridge where he was working on the speciation of the itch mites *Sarcoptes* in Nuttall's laboratory. He also took the course in tropical medicine in London—where, according to tradition, he was chiefly noted for his habit of arriving late and reading the newspaper during lectures—characteristics which in later times he found impossible to tolerate in students. Which goes to show how our point of view changes with the years!

In March 1921 Buxton accepted the post of entomologist in the Medical Department in Palestine. Here he remained for two and a half years devoting

himself on the one hand to the survey of malaria and other parasitic diseases (sound but unenlightened work according to his own assessment—for he had no contact with anyone who really knew about malaria) and on the other to the study of desert and semidesert faunas. The output from two years work was remarkable and culminated in the production of what he himself has called that 'vigorous young man's book' on *Animal life in deserts*.

By the summer of 1923 Buxton was beginning to feel that he had been enough in Arab and Persian lands and that if he was to be a real medical entomologist he ought to see the damp tropics. This opportunity came when he accepted a temporary post under the London School of Tropical Medicine to lead a research expedition to Samoa. The purpose was to continue the Manson tradition and to pursue the study of filariasis. The plan was somewhat vague: perhaps to control the insect vector, perhaps to do work on its biology. Anyhow, he had plenty of equipment, generous terms and complete independence; so that the vagueness of plan was probably what he would have wished. He chose his own assistant, G. H. E. Hopkins, a careful worker and a good naturalist, and together they spent two years in Samoa.

During that time an immense amount was accomplished. The general biology of *Aedes scutellaris* (*variegatus*) which transmits the infection was known from the work of Manson-Bahr and others, but they carried out a great many simple experiments in an attempt to analyze the specialized selection of breeding places by the egg laying female—a line of study which has been much developed since but was new at that time. Buxton also visited the New Hebrides for a few months and made a useful survey of the general incidence of malaria, filariasis and other diseases, and he spent a month touring the Ellice and Tokelau Islands in a mission ship. In this way he was able to study a true sample of Oceania and became familiar with the ways of life of Polynesians and Melanesians in the widely dissimilar types of island. All these surveys and experiments were published in two large memoirs by the London School of Hygiene and Tropical Medicine.

As a side line Buxton and Hopkins collected Samoan insects. Instead of the very restricted fauna that would all go into 'two or three cigar boxes' which some entomologists had foretold, they collected about 1600 insect species, many of them new to science. All were eventually listed or described in a series of memoirs on *The insect fauna of Samoa* and published in good style by the British Museum. All this amounted to a substantial achievement which entailed the co-ordination of the work of experts in all parts of the world, and in the result the insects of Samoa were better known than those of nearly all the other island groups in Oceania.

The Pacific trip lasted from the end of 1923 to January 1926. Buxton was beginning to feel that it was about time to settle in England. In January 1917 he had married Muryell Gladys Rice, daughter of Rev Hon. William Talbot Rice (the son of a Baron Dynevor—a family of Welsh landowners and coal-mine owners) and Marian Gurney. Buxton and his wife were therefore second cousins and doubtless related in other ways, their large connexions

having intermarried frequently and over a long run of years. They had gone out to Palestine with one small baby, a son, and returned with two. A daughter was born in Samoa. These simple statements will perhaps serve to convey the extent to which Buxton was indebted to his wife during these rather hectic years. Though not sharing his scientific interests she always provided a home and a background and shared to the full his interest in the places and people around them.

By the time Buxton was back in London in January 1926 he had been appointed head of the Department of Entomology in the new London School of Hygiene and Tropical Medicine, which had absorbed the old London School of Tropical Medicine and was now gathering itself together under Andrew Balfour. The great new building in Gower Street was only on paper, and in an early state. Several departments (bacteriology, epidemiology) had not yet been formed and when formed had to be housed separately, in several parts of London.

The Department of Entomology in the old School had been the creation of Colonel A. C. Alcock—an able and scholarly medical entomologist, much loved and respected, but sadly aged by the time of his retirement. All the departmental letters, many of them unopened, were kept in a soap box. There was not one single piece of graduated glassware, no balance. But, with the large Rockefeller donation, there was plenty of money available for equipment.

At this time Buxton wrote a letter to *Nature* in which he put forward the view that 'real progress will not be made in applied entomology until . . . we devote time and labour to the study of the fundamentals of insect physiology'. Following this up he persuaded the Board of Management that 'the contribution of the School to medical entomology should be to go deep and to produce a science of insect physiology'—as a basis for the understanding of insecticidal action and for the interpretation of biology in the field. Walter Fletcher, who was now Secretary of the Medical Research Council and a very active member of the Board of Management of the School, strongly supported Buxton in this point of view.

In advertising for an assistant, with a very wide range of desirable qualifications, a special point was made of experience in biochemistry or some side of physiology—a highly original requirement in those days when filling a post in medical entomology. Among the applicants was V. B. Wigglesworth, 'who had had a good academic career, had written half a dozen papers, some jointly with J. B. S. Haldane, was about to qualify in medicine—and was very silent and unimpressive at interview. Against him were middle aged people, with lots of field experience in anti-malarial work etc.' Buxton, very young and very definite, strongly supported Wigglesworth at the appointing committee; he was backed up by Walter Fletcher and the appointment was made—although, as Balfour did not hesitate to point out to the successful candidate, it was a very queer one. These facts are perhaps worth putting on record because Buxton always rightly felt that one of his most valuable contri-

butions to entomology was the impetus he had given to the study of insect physiology, and this was his first step. In this way began the happy (though not uncritical) association between the present writer and Patrick Buxton that was to last for nineteen years.

There followed the busy period of planning and equipping the new Department, of building up the teaching courses in medical entomology, and of putting the idea of research in insect physiology into practice. Buxton himself took up the study of the relations of insects to climate. Their water and temperature relations. More will be said about this when we come to discuss Buxton's scientific contributions; it was the general field of research that occupied his attention between 1926 and 1939. Other appointments were made, the most notable of which were that of K. Mellanby who joined Buxton with the support of the Medical Research Council to extend the work on water and temperature relations—which he did to very good purpose—and R. P. Hobson whom the Empire Marketing Board financed to study digestion and nutrition in *Calliphoridae*.

By 1931 Buxton began to feel the need to travel once again. He made a brief trip to Palestine in an attempt to study the microclimate in which the fleas of rodents actually exist in hot dry countries. In 1933, in company with a young graduate D. J. Lewis, a distant cousin, he undertook a more serious expedition of seven months in Northern Nigeria where they carried out extensive observations on the climatic conditions in which the several species of tsetse flies develop in this hot dry part of Africa. In this same year he was made Professor in the University of London.

Throughout these years a long series of entomologists from all parts of the tropics visited the laboratory for short periods. And many others, after a period of training in laboratory research, have had distinguished careers as medical entomologists: D. J. Lewis, R. C. Muirhead Thomson, A. Haddow, E. P. Hodgkin, and many more. In medical entomology the most important enterprises were a detailed investigation of the physiology and ecology of the bed-bug, carried out by C. G. Johnson with the support of the Medical Research Council (1935-1940), a study of the factors concerned in the spreading of anti-malarial oils by D. R. P. Murray supported jointly by the major oil companies (1936-1940), and an analysis of the egg-laying behaviour and biology of *Anopheles minimus* in Assam by R. C. Muirhead Thomson under a grant from the Royal Society.

For a year or two in the late nineteen thirties Buxton had been working on a projected text-book of medical entomology. One chapter was finished, that on the louse. On the outbreak of war in September 1939, thinking, as many did, in terms of the earlier world war and picturing again a stationary warfare in trenches, Buxton decided to devote the main effort of his department to the improvement of practical measures against the louse. The chapter in the book was rapidly expanded to form an up-to-date monograph on the subject and within seven weeks of sending to the press it was published.

Twenty years had gone by since there had been any serious work on the

control of lice. The accumulated experience of twenty years in applied entomology was available for application in this field. Buxton appreciated that the great shortcoming of all the de-lousing procedures developed during the 1914-18 war was that they had no lasting effect: immediately the man returned to the trenches he was re-infested. The answer appeared to be a 'lasting' insecticide with which the clothing could be impregnated. There had been considerable advances in insecticides in the course of twenty years and particular attention was given to the organic thiocyanates.

Buxton's methods of conducting this investigation were energetic, unconventional and Buxtonian. They were not very popular with the authorities of the London School of Hygiene. By means of contacts with relieving officers and the keepers of common lodging houses, he built up a clientele of verminous tramps who (for a small consideration) used to flock to the 'louse clinic' in the School. Here they were given new shirts for old, or their shirts were sprayed or dusted with various new chemicals. Meanwhile everyone possible, including most of Buxton's family, were pressed into the service of rearing lice in small containers under their socks or vests to provide the material for controlled experiments in the laboratory.

The unsavoury and unconventional aspects of this enterprise appealed to the showman in Buxton; but it was all most useful work. Only one thing was lacking—a suitable insecticide. The thiocyanates were effective in killing lice but they were too irritating to the human skin for general use. On the other hand, incorporated in a hair oil, they proved invaluable in the treatment of head lice which were a major problem at this time both among the children evacuated from the large towns and the girls recruited to the Women's Services. He developed a pleated impregnated belt which could be worn beneath the clothing—but this also proved too irritating during sweating—as became only too apparent when a certain Highland regiment carried out an exercise on the Cairngorms wearing the impregnated belts.

By 1942 this work had reached a deadlock. Then we were approached by Dr G. A. Campbell of the Geigy Chemical Company in Manchester (he had been referred to us by Dr F. Tattersfield) with the report that their parent company in Switzerland had produced a new synthetic insecticide which they wished to make available to the western allies. We read the patent specification of 'Gessarol', and were frankly sceptical; for this material, non-irritating and apparently harmless to man, non-volatile and long lasting, highly toxic to insects, was exactly what Buxton had visualized as the lasting insecticide against the louse. Of course the material had to be tested. A small sample was synthesized at Manchester; a kilogram sample was obtained from Switzerland. Both had the same properties; the material was even more promising than we could have hoped. Almost at the same time the branch of Geigy in the United States brought the new insecticide to the notice of the American authorities. They were equally impressed. Soon afterwards, at a meeting of the Insecticides Research and Development Committee, the Chairman, Professor (later Sir Ian) Heilbron christened the new insecticide

'DDT'. The name was accepted by the Americans and the rest of the story, which marked the beginning of a new era in applied entomology, is well known. Buxton's preparatory work proved very useful in the early exploitation of DDT.

Buxton's interest and energy was by no means confined to the louse problem. On the outbreak of the war he established close relations with the medical departments of the War Office, Admiralty and Air Ministry. He organized series of lectures to nurses and shelter marshals. He persuaded Sir Edward Mellanby as Secretary of the Medical Research Council to set up an Entomological Sub-Committee of their Military Personnel Research Committee, with Buxton as chairman. This proved a useful medium during the early war years for advising the Service Departments on modern methods in medical entomology. It was not so effective as it might have been for it had no means of ensuring that its recommendations were carried out. That defect was later remedied by an ingenious administrative device in a new committee, the Pyrethrum Development Panel (later the Insecticides Research and Development Committee) under the chairmanship of Professor Heilbron. It was characteristic of Buxton that he was not interested in the slightest degree in his personal position in these affairs. He was concerned only in improving the methods of medical entomology in the Services.

Buxton certainly enjoyed his war. It provided all the opportunities he could desire for deploying his unbounded energy. Work was initiated on fumigation against lice in clothing and bedding, on mosquito repellents, on mosquito and fly sprays, on the control of bed bugs in air raid shelters, on the control of head lice and other projects. He made trips to the United States as a member of a mission from the Ministry of Production to agree on the sharing of pyrethrum supplies, to Egypt to try out the louse belts (which proved of real value when used among native labour forces in the Eastern Mediterranean), to West Africa to take part in trials of aerial spraying against mosquito larvae. His ambition of making a trip to Russia to demonstrate the new methods of controlling bed bugs and body lice was thwarted only by Madame Maisky (the wife of the Russian Ambassador), who solemnly assured him that neither of these insects existed any longer in Russia.

At the end of the war in 1945 when Buxton had to re-start his department on a peace-time footing, he felt disinclined to go back himself to physiological research. He even suggested more than once in conversation that the study of insect physiology had not yielded quite what he had hoped. But he attracted workers in many sides of medical entomology and his department was never more active. It was at this time that as a member of the Colonial Medical Research Committee (of the Medical Research Council jointly with the Colonial Office) Buxton was invited to carry out two extensive tours in East Africa, from Kenya and Uganda to Zululand and Bechuanaland, to report on the tsetse fly and trypanosomiasis with reference to the organization of research and control. These long journeys occupied much of 1945 and 1946.

The last decade saw the building up of the Department of Entomology as a most active centre of teaching and research. Buxton became increasingly in demand as an adviser and as a member of committees. He had been elected a Fellow in 1943 and served on the Council of the Royal Society for 1951-53. He was appointed a member of the Medical Research Council in 1945 and served on their Committees dealing with Typhus, Malaria and Colonial Medical Research, as well as the Tsetse Fly and Trypanosomiasis Committee of the Colonial Office. He served on the Council of the Royal Society of Tropical Medicine and of the Royal Entomological Society, of which he was President in 1942 and again in 1953-55. He was made a C.M.G. in 1946. In 1949, on the occasion of the jubilee of the Liverpool School of Tropical Medicine, he was one of the recipients of the Mary Kingsley Medal. In 1951 he received a Linnean medal and in 1953 the Gold Medal of the Linnean Society. In 1950 he was elected to Foreign Membership of the Société de Pathologie Exotique. During these latter years, however, Buxton continued to travel: to the Southern Sudan to study tsetse flies, to Canada to see the black-fly and mosquito problems of the far north, to Central America to gain first-hand knowledge of an epidemic of jungle yellow fever, and to Malaya. Indeed, between 1940 and 1954 there was no year in which he did not pay at least one visit to the tropics or sub-tropics.

Scientific work

The most striking feature of Buxton's output of scientific research, extending over forty years, is its diversity. There are papers dealing with pure taxonomy and descriptive entomology, papers on applied entomology in agriculture and medicine, on insect physiology and ecology—particularly on the border line between these two, where the study of physiology in the laboratory and of the micro-climatic conditions in the actual environment serves to define the limitations to the existence of a given insect in the field. Interspersed with these are papers on the birds or on the people in the many lands he visited, on geography and ethnology.

During the period of his most intensive concentration on research, the twelve years before the second world war, Buxton's main interest was in those physiological factors which determine the survival and increase of insect populations under different conditions of climate, particularly the effect upon them of different temperatures and humidities. He made clear that for many purposes the saturation deficiency of the air is the most informative measure of its drying power in relation to the insect, and he repeatedly emphasized the importance of considering the 'micro-climate'—the climate in the actual spot when the insect rests—rather than the climate of the meteorologist.

In my view, however, Buxton's claims to distinction do not lie in the realm of experimental science. Without question he did useful work in that field. His experimental work on the relation of insects to climate is not negligible

in itself and its influence on the work of others perhaps even more important, both in his immediate circle and among entomologists in general. His contributions to the applied entomology of filariasis, malaria and the louse-borne fevers were imaginative and timely. But these do not by themselves represent an outstanding achievement.

It is probably true to say that, except in so far as they contribute to theories and generalizations, the scientific mind is not interested in facts. But for Buxton, I believe, the scientific theory had less appeal than the facts themselves. This attitude is characteristic of the naturalist; and in my opinion it is as a naturalist that Buxton was outstanding. Whatever he might be doing, in whatever part of the world he might be, he would be sure to produce observations on the local natural history of great interest and originality. His war-time experiences in Persia and Iraq, and after the war in Palestine, resulted in the production of his book on *Animal life in deserts*. This has become a classic; and after a continuous demand lasting for thirty years, it was reprinted in 1954. In Samoa, apart from the extensive field work required for the *Insects of Samoa*, Buxton made the remarkable discovery of a truly marine Chironomid midge, *Pontomyia natans* Edwards, in which not only is the larva adapted to developing in salt water (a phenomenon not entirely without precedent) but the adult midge itself is a submarine creature which swims actively in the strong tidal currents.

In every country he visited, Buxton contributed useful observations on the birds and plants; and these catholic interests extended to the human populations. In 1921 he was writing on the way of life of the Marsh Arabs of Lower Mesopotamia; in 1926 and 1929 he writes a sympathetic account of depopulation in parts of Melanesia and Polynesia. And his more professional studies on the incidence of malaria and filariasis in the South Pacific are illuminated at every point by an understanding of the habits and outlook of the local population.

This feeling for natural history, ethnology and geography was coupled with an interest in language. Buxton early mastered Arabic and this helped greatly in putting him *en rapport* with Arab people both in the Iraq and Palestine days and later in Egypt. And when in Samoa he went some way towards mastering that difficult language—the ‘pidgin’ of the South Seas.

These wide interests in anthropology, geography and animal and plant ecology provided the ideal foundation on which to base a really comprehensive account of the tsetse fly problems of tropical Africa. Before the war of 1939 Buxton had started on an ambitious text book of medical entomology. It was never completed. The chapter on the louse was quickly turned into an admirable practical monograph and published as *The louse* soon after the outbreak of the war. The chapter on the tsetse fly grew into the great memoir of 800 pages published by the London School of Hygiene and Tropical Medicine in the last year of Buxton's life.

This *Natural history of the tsetse flies* is Buxton's *magnum opus*. Going out to Northern Nigeria for some seven months in 1933 with D. J. Lewis, he had had

some first-hand experience of two species of *Glossina* under field conditions, and had made a substantial contribution to their physiology in relation to climate. This work served to emphasize that in much of its range the tsetse fly is on the border line of existence so far as climate is concerned; it is able to maintain itself only because the 'micro-climate' is within the favourable range. This discovery has provided a useful lead in the control of the tsetse fly. As T. A. M. Nash, K. M. Morris and others have found, when the undergrowth in the thickets is cleared away, without touching the larger trees, the climate in the actual spots where the tsetse fly lives and breeds becomes too unfavourable and the fly dies out. Then Buxton's extensive tours in East Africa on behalf of the Colonial Office during 1945 and 1946 gave him the opportunity of seeing the very different and diverse conditions in those regions, at first hand, and in the company of people who had been studying them for many years. Bringing his fresh outlook and wide experience to bear, and reading widely in the copious literature of the subject, he was able to achieve a masterly synthesis, in which the physiology and ecology of the many species of tsetse flies are linked with the climatic and vegetational characteristics of the various regions, and these again with the ways of life of the diverse peoples and with the incidence of sleeping sickness and trypanosomiasis in domestic animals. This book combines in a remarkable degree an immense amount of local detail with the guiding principles and generalizations derived from ecological studies. In view of Buxton's early advocacy of the study of insect physiology it is good to see the extent to which laboratory work on the tsetse fly has provided a basis for understanding its ecology.

Personal record

Patrick Buxton had a strong and distinctive personality. Completely honest, with definite personal views, he was a man to be relied upon. But for many who came into contact with him the most notable feature about Buxton was his manner—the well-known 'Buxtonian manner', a characteristic mixture of irony and display, infused with a quick wit and a well-developed sense of humour. His sharp sarcastic tongue was well known in committee. I always supposed that this manner, which could strongly antagonize those who did not see beyond it, was a product of his school days, an intellectual method of protection against a rough world. But very many people, in all parts of the world, were not misled by this odd veneer and Buxton had friends everywhere. For the real and outstanding qualities in Buxton's character were his honesty of purpose and his thoughtfulness for others—a deep unconscious loyalty to a long family tradition.

Buxton's interests were at once broad and narrow. He was, as we have seen, intensely interested in all sides of the natural history of plants and animals, in geography, meteorology, and in the life of primitive peoples and their languages. At home he was an enthusiastic gardener, cultivating rare and unusual plants in his fine garden at Gerrards Cross and never happier

than when showing them to the many friends who were invited down there or digging them up for these friends to take away. He usually wore an uncommon flower or a sprig from some rare plant in his buttonhole.

Outside his special subjects Buxton found it difficult to be interested. Music he disliked, and anyone who was interested in poetry or philosophy he would regard as distinctly abnormal; he was much attached to and well informed about the arts and crafts of Persia, less sympathetic to other forms of art. In science itself he always seemed to me to be more fascinated by the multifarious details that appeal to the naturalist than by the ideas and generalizations that are the objectives of the scientist and philosopher. In the day-to-day pursuit of his researches he usually preferred to collect facts and to sort them out with the aid of statistics than to devise experiments.

Buxton, as this record has set out to show, was a great naturalist; and in common with that rare breed, he had a fine command of English and wrote in a lucid unaffected style. Even in his massive work on the tsetse fly the high quality of the writing is maintained and will ensure that this book has an honourable future. He was equally effective as a speaker, with a vivid descriptive power and a way of presenting even familiar matters in a new light.

Behind all his professional activities Buxton was a family man. Happily married at an early age, he had a family of two sons and four daughters. These have taken up very diverse careers. Only one of them, the second boy, Andrew, became a professional biologist. Andrew Buxton bore a striking resemblance to his father in general appearance and in his quick wit and lively sense of humour, so that people knew on sight whose son he was. After running the Natural History Society at Rugby and graduating at Trinity College, Cambridge, Andrew Buxton had started on research in East Africa, studying the natural history of monkeys and other potential reservoirs of the yellow fever virus—when he was suddenly struck down by poliomyelitis. The death in 1952 of this son in whom he had been able to re-live his own enthusiasms was a terrible blow to Patrick Buxton. It seemed for a time that he would never recover, and when he did so it was apparent to all his friends that many of Buxton's asperities had gone and the fine quality of his nature was there for all to see.

It is not usual to use the word tragedy when writing of the death of a man of sixty-three whose main work has been accomplished. But in his last few years Buxton was turning his attention to a new branch of natural history, that of the Mycetophilidae—with the idea of providing a new field of interest for his retirement. He was already producing valuable results and when he died on the threshold of what promised to be a new career, those who had appreciated his kindness and unselfishness for many years felt his death as a tragedy.

V. B. WIGGLESWORTH

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J. F. Buckley

SHELDON FRANCIS DUDLEY

1884-1956

SHELDON FRANCIS DUDLEY, destined to become Medical Director-General of the Royal Navy in the second World War, was born under the care of his future Service in the Royal Naval Sick Quarters, Lisbon, on 16 August 1884. His father, John Dudley, was a medical officer in the Royal Navy who, when he retired in 1906, had reached the rank of Deputy Inspector-General (equivalent today to the rank of Surgeon Captain). The Dudleys were a Quaker family, whose forbears were alleged to have settled in Co. Tipperary, Ireland, at the time when the Pilgrim Fathers fared further west to America. Sheldon Dudley's uncle was for many years Protestant Rector of Glenarm, Co. Antrim. His mother was Edith Bella, eldest daughter of Richard Whittington, Prebendary of St Paul's and Rector of St Peter's, Cornhill, who was said to be a direct descendant of Robert, a brother of the famous Dick Whittington. Prebendary Whittington was well-known in the City of London and was for a time Master of the Merchant Taylors Company and twice Chaplain to the Lord Mayor. Dudley had one brother, two years his junior, who was killed on the Somme in the 1914-18 war. In 1913 Dudley married Ethel Edith Wood (*née* Franklyn), a widow with one son. There were no children of the marriage. Dudley died on 6 May 1956, and is survived by his widow.

When he was three years old Dudley was brought home to England by his parents, and three years later accompanied them overseas again—this time to Yokohama, where his father became Principal Medical Officer to the Royal Naval Sick Quarters. At this early age the boy began to evince an interest in natural history, and was taught by his father how to use a microscope. Returning home to Chatham in 1893, he was sent to a 'dames' school, where he says he learnt little, but, during his holidays, he pursued his hobby of studying pond life and the common objects of the sea-shore—a hobby which on one occasion led to his being punished for dissecting a fish on the dining-room hearth-rug. When he was eleven he entered Merchant Taylors' School, and about this same time he met Sir Albert Seward (later Foreign Secretary of the Royal Society), who was a connexion by marriage of the Whittington family. Seward fired young Dudley's interest in research by his talks on palaeobotany, and allowed him to examine sections of fossil plants at the Natural History Museum in South Kensington. At first, Dudley was on the Classical side at Merchant Taylors' and, except in mathematics, he did badly, having no flair for languages. Because of this, his parents—who had hoped

he would in due course enter the Church—reluctantly had him transferred to the Modern side. In this new environment he quickly found himself at home and became much interested in science—physics, chemistry and biology. He passed the London Matriculation examination at the age of fourteen, being the first boy ever to pass the Preliminary Science examination straight from school. As a reward for this, he was reinstated on the ‘Prompter’s Bench’ (Prompter at Merchant Taylors’ was a junior monitor) from which he had previously been publicly ‘disrated’ for smoking in class. At the end of his school career, while in the Sixth Form, he distinguished himself by winning the ‘chief science prize’ and a scholarship to St Thomas’s Hospital, for which he had competed in pursuance of his ambition to qualify in medicine and enter the medical service of the Royal Navy. He began his studies in St Thomas’s Medical School in October 1901, and found no difficulty in qualifying as M.R.C.S. and L.R.C.P. in the normal period of five years. He joined the Royal Navy in November 1906 without taking any house appointments, following the advice of his father, who said that if it was his intention to enter the Navy the sooner he got in the better. In fact, the father, although a naval officer himself, was opposed to the career that his son had selected. He did not want young Dudley to ‘waste his life’ in the medical service of the Royal Navy, and said that, if he insisted on doing so, he (the father) saw no reason to spend more than the minimum on his medical education. Dudley regretted that this decision made it impossible for him to go to Cambridge or take on house appointments, as he otherwise would have done—but he never regretted entering the Navy.

Among the teachers at St Thomas’s Medical School, those who made the greatest impression on him and played a considerable part in shaping his subsequent career, were Shattock (pathology), Dudgeon (bacteriology) and Sir Cuthbert Wallace (surgery). They influenced him because they were not only good teachers but also active research workers in their different spheres. He won a few unimportant distinctions and prizes as a medical student, but he had little interest in the routine type of work required for passing examinations, which he regarded as a necessary evil. In fact, he tended to neglect study of this kind, and spent much of his time in desultory reading and in the study of certain aspects of microbiology which had little to do with the examinations of the standard medical curriculum.

Despite his studious habits, young Dudley was no mere bookworm, being above the average at games. At the age of sixteen he was six feet tall (he ultimately grew to six feet one and a half inches), and at a weight of thirteen stone was a ‘good bustling forward’ at football. He played both football and water-polo for St Thomas’s Hospital and toured with the Old Merchant Taylors’ XV at Easter and Christmas. After joining the Navy he played for the United Services at Plymouth, but did not get his Navy cap. Though naturally good at games, his main interests lay elsewhere and he did not take sport seriously enough to devote much time to training. He was a first-class swimmer, pulled a good oar, and was proficient in handling small sailing boats.

NAVAL CAREER

Of the thirty-nine years which Dudley served in the Navy, twelve were spent at sea. In the interval before the first war he served successively in the Mediterranean, China and Home Fleets, latterly in H.M. Hospital Ship *Maine*. At the beginning of the first world war he was pathologist and venereologist at the Royal Naval Hospital at Chatham. Later he was appointed Senior Medical Officer of the Royal Naval Aircraft Depot, Dunkirk, then of H.M. Hospital Ship *Agadir*. In 1919 he was made Pathologist at Haslar, and in 1921 became Professor of Pathology at the Royal Naval Medical School at Greenwich, an appointment which he held for three years. At sea again, he spent a year as Senior Medical Officer of H.M.S. *Barham* in the Mediterranean Fleet, and thereafter was posted to the New Zealand Station, where he made so good an impression on his colleagues ashore that he was approached by Otago University in connexion with the Chair of Pathology, which however he refused. He returned to Greenwich in 1927 and, on promotion to Surgeon Captain in 1930, became Director of Medical Studies and Professor of Naval Hygiene at the Medical School and later Medical Specialist at Chatham. In 1936 he was promoted Surgeon Rear-Admiral and became the first Deputy Medical Director-General. In 1938 he was made Medical Officer in Charge of the Royal Naval Hospital, Chatham, and in July 1941 he took over the appointment of Medical Director-General, with the rank of Surgeon Vice-Admiral. He filled this onerous post, with an extension of the normal period of tenure, until the war was over, and then—to quote his personal record—posted himself, in the rank of Surgeon Captain, as Principal Medical Officer to the Royal Naval Hospital, Bermuda, where he ‘became an ordinary clinician or G.P., opening sailors’ bowels and whitlows with great zeal after fifteen years of more or less administration work in a bureau’. He returned home and retired in 1947, his main occupation thereafter being the writing of his two books, *The four pillars of wisdom* and *Our national ill health service*. He made a partial return to public life in 1951, when he acted as medical adviser to the Science Director of the Festival of Britain, and wrote scripts for displays of medical and health subjects.

FURTHER QUALIFICATIONS AND DISTINCTIONS

Dudley made good use of his periods of home service to obtain higher medical qualifications. In 1920 he took the M.D. (London) and D.P.H., and in 1921 the D.T.M. While studying at the London School of Hygiene and Tropical Medicine he won the Duncan and Lalcacca Medals and in 1922 was awarded the Gilbert Blane Medal, a special adjudication for conspicuous zeal and professional ability. In the same year he won the Liddell Triennial Prize of the London Hospital for a paper on the epidemiology of influenza, and in 1923 an essay on the carrier problem gained for him the Neech Prize of the Society of Medical Officers of Health. After returning from overseas service he took his M.R.C.P. in 1928, and was elected to Fellowship in 1933.

He was given the Chadwick Prize and Gold Medal in 1930 in recognition of his work on diphtheria immunization, and in 1931 was Milroy Lecturer of the Royal College of Physicians, his subject being 'Lessons in the distribution of infectious diseases in the Royal Navy', for which he analyzed the figures given in *The health of the navy* from the date of the inception of this publication.

In these years he held various offices in learned societies. He was President of the War Section (now defunct) of the Society of Medical Officers in 1929, and of the War Section (now the United Services Section) of the Royal Society of Medicine in 1930. He was President of the Section on Epidemiology and State Medicine of the Royal Society of Medicine in 1935 and 1936, and Vice-President of the Royal Society of Tropical Medicine and Hygiene from 1939 to 1941.

His first Service decoration, which he received in 1919, was the O.B.E. In 1939 he became Honorary Surgeon to the King and in 1940 was awarded the C.B., and subsequently in 1942 was elevated to K.C.B. In recognition of his services to the naval medical interests of the United States during the second World War he received the Legion of Merit, Degree of Commander, of U.S.A., 1946. The citation for this honour is as follows: 'Vice-Admiral Sir Sheldon Dudley, British Navy, performed outstanding service from July 1942 to May 1945 as Medical Director-General, Royal Navy. He demonstrated unusual co-operation in placing at the disposal of the United States Army the facilities of British Naval Hospitals. He was instrumental in arranging for the use of two naval hospital ships to support our landings in North Africa, Sicily, and Italy. He played an important part in assisting the United States Army Medical Corps during the planning period of the invasion of France. Vice-Admiral Dudley's work materially assisted the joint Anglo-American war effort and reflects great credit on himself and on the British Navy.' (Dudley writes that this was a most anxious responsibility; the U.S. hospital ships which had been promised failed to be ready in time. He was gratified that the U.S. authorities realized the risk he accepted, i.e. that the diversion of hospital ships might let our own troops down). He was also made Commander, Legion d'Honneur, France, 1947, Grand Commander of the Order of St Olaf, Norway, 1947, Knight Grand Cross of the Order of Orange Nassau, Netherlands, 1947.

He was elected to Fellowship of the Royal Society in 1941 (it was ninety years since an officer on the Active List of his Service had been so honoured), and was made an Honorary Fellow of the Royal Society of Medicine in 1945. In 1946, the Royal College of Surgeons, Edinburgh, conferred on him an honorary Fellowship, and in 1953, on delivering the Lind Oration at the bicentenary celebrations in Edinburgh, he received the honorary degree of LL.D. of the University of that city. In 1947 he was admitted to the Freedom of the Worshipful Company of Barbers. As a youth he was 'apprenticed' to his grandfather, Prebendary Whittington, as a Merchant Taylor, and in 1949 he became a Freeman of the Merchant Taylors Company and also of the City of London.

THE RESEARCH BACKGROUND

In the services, 'pathology' is a comprehensive term, and in Dudley's young days the holder of an appointment in this subject had to be morbid anatomist, bacteriologist, serologist, haematologist, and to a lesser extent chemist and biochemist. He was, moreover, expected to be a clinician of no mean standing and, in all matters pertaining to laboratory diagnosis, to be guide and counsellor to the young medical officer first faced with the problems of infectious disease peculiar to community life. Dudley's training and experience in 'pathology' under diverse conditions during his early years in the Navy, coupled with his natural leaning towards mathematics and statistical analysis and his ruminating philosophical approach to the problems which confronted him, led almost inevitably, in the environment in which he served, to his choice of epidemiology as a subject for detailed study—he himself defined his special bent as 'bacteriological epidemiology'. Research was his hobby, and he wisely selected as his subject problems which lay ready-made, to hand, and which were of outstanding importance to the Service in which he passed his life. Reviewing his work and his many publications, one feels, however, that his interests were wider than mere epidemiology and dealt on a broad basis with the preservation of health and with human ecology, two terms which occur frequently in his later writings.

His appointment to Greenwich in 1921, bringing under his supervision the pupils of the Royal Hospital School, afforded him an unrivalled opportunity to study the problems of infectious disease, and marked the beginning of a long period of fruitful research which had far-reaching results. The pupils of this school, mainly the sons of naval ratings, were aged from eleven and a half to fifteen and a half years. They came from all over the country, and joined usually in two separate groups each term, there being the usual three terms in the year. Detailed medical records were maintained of each boy, and it was possible from these to follow the incidence of sickness in each of the three annual batches from beginning to end of their school career. The boys stayed on an average for three years before being drafted to various naval training establishments. There were up to about one thousand boarders and, in addition, some one hundred day-boys. The hygienic conditions and sanitary discipline were good, and water supplies, food, and milk were carefully supervised. The boys had to pass a severe medical test before entering the school, and during their years of residence were kept under strict medical supervision, including frequent examinations of the throat and nose. Although the numbers who slept in the dormitories were high, the space per boy was adequate, and there was no overcrowding, according to accepted standards. In spite of all this, the incidence of certain bacterial diseases, and particularly diphtheria, was considerable. Here was an almost ideal 'experimental herd' (a phrase which Dudley once used incautiously in public, with the result that a question on the ethics of making helpless schoolboys the subject of experiments was asked in the House of Commons!), and in the years until the school

was moved elsewhere in 1933, broken only by his period of four years' service at sea, he made full use of the opportunities which it offered.

In his later years of service, Dudley's approach to research was, of necessity, less direct, but his interest did not wane, and, if he was personally unable to undertake clinical or bench work, he associated himself with many important investigations, and lent them the full weight of his influence.

Diphtheria

When Dudley was posted to Greenwich in 1921, the incidence of diphtheria among the schoolboys was the cause of much anxiety. Following an outbreak in which there were sixty-one cases in the first term of 1919, there had been a period of comparative freedom, though sporadic cases occurred, until the second term of 1921, when a further outbreak started. This continued throughout the third term, and produced a total of ninety-four cases. All efforts to check the spread of the disease by the methods which were then standard—frequent inspections, throat swabbing, and the segregation of carriers and suspected cases—had proved of no avail. Authority was given to make use of the recently devised Schick test to determine those who were immune and those who were susceptible, and Dudley proceeded in 1922 to carry out a carefully standardized and controlled test on the 831 boarders who were resident at the time. Comprehensive records were available which gave a reliable medical history of all these boys from the time they joined the school, and these enabled him to trace the development of immunity. He approached the problem by dividing the boys into batches according to the term in which they entered the school, and worked out the percentage of immunes—that is, Schick-negatives—in each batch. In this way he was able to relate the rising level of immunity to the length of residence in the school and exposure to the greater risk of infection which existed during the outbreaks. As would be expected, the percentage of immunes in the batch of boys who had been longest in residence was considerably higher than that in the latest recruits. However, when the immunity level was plotted in terms of batches, the resulting curve did not follow a steady course: there were two well-marked steps, each related to the outbreaks of clinical diphtheria, with an intervening period in which a constant level was maintained. Thus, in the new entrants, the level was about 60 per cent. In those batches who had been in the school during the 1921 outbreak but not during the 1919 one, that is, in boys who had been resident from six months to two and a half years, the level was about 85 per cent., while in those who had weathered both outbreaks the level was 95 per cent. Thus there was clear evidence of a substantial rise in the immunity level at the time of, or immediately following, each outbreak of clinical diphtheria. Any possibility that the increased immunity was related to the age of the boys was ruled out by rearranging the figures in age-groups, when there was found to be no significant difference in the percentage of immunes in the different groups.

A careful study of the behaviour of the Schick reaction in clinical diphtheria revealed the rather surprising fact that it was frequently positive in convalescents. In other words, clinical cure had occurred although the circulating antitoxin was below the level required to neutralize the test dose of toxin. However, although these boys continued to be exposed to infection, none of them suffered from a second attack of diphtheria, and a fairly rapid conversion of the reaction took place, so that within nine months 92 per cent had become Schick-negative. During epidemics the carrier-rate was high, and the degree of exposure to infection was therefore much greater than in the inter-epidemic period.

From a consideration of these facts, Dudley concluded that the higher level of immunity in boys who had been in the school during an outbreak of clinical diphtheria could be explained only by the occurrence among them of latent or sub-clinical infection—probably resulting from exposure to doses of bacteria too small to become established and to produce overt symptoms. He considered that his figures afforded experimental proof of this conclusion, and in later years regarded this as his most important contribution to science, because of its wide implications in the general field of immunology.

In carrying out this experiment and in others of later date, Dudley made many useful and practical observations on the interpretation of the Schick test. He showed that pseudo-reactions, which are liable to cause much confusion in the absence of adequate controls, are due to an allergic condition of the skin caused by previous contact with diphtheria bacilli; this allergic condition may develop in a relatively short space of time—a matter of a few months—and may be, although it is not invariably so, a step on the way to immunity. He found that, while carriers of virulent diphtheria bacilli were almost always Schick-negative (sporadic isolations of virulent bacilli might obviously be made from Schick-positives undergoing the latent infection destined to convert them to the Schick-negative state), carriers of avirulent bacilli might be either positive or negative. He made a careful study of the incidence of virulent and avirulent diphtheria bacilli in the boys' throats and of their significance in carriers. He found no constant ratio between cases and carriers, and on occasion noted a high carrier rate of virulent diphtheria bacilli in the absence of clinical diphtheria and in the presence of Schick-positives. He concluded that all Schick-positives were not equally susceptible to infection and that some of them were probably immune to ordinary doses of diphtheria bacilli. He observed that, in carriers, virulent organisms were frequently replaced by avirulent, and he advanced the hypothesis that Schick-positives, when infected with virulent diphtheria bacilli, might suffer latent infection during which the virulent bacilli became non-virulent. He concluded that, at one time or another, 75 per cent of the boys must have been carriers, and that, over all, for every twenty carriers of virulent bacteria there was only one case of clinical diphtheria.

In 1928, largely as the result of Dudley's influence, authority was given for the active immunization of all boys in Greenwich Hospital School who

gave a Schick-positive reaction, and Dudley was able to carry his work to its logical conclusion. The immediate result, as shown by the incidence of clinical diphtheria, was dramatic. From 1919 to 1927 inclusive, there had been over four hundred cases. From the inauguration of active immunization in 1928 until October 1932, only twenty cases (including 'sore throats' with 'morphological diphtheria bacilli' in the throat swab) were reported. Of these, only four were recognizable as clinical diphtheria, and three of them were Schick-positive new entrants who had not been immunized. There was, however, a recrudescence in the last term of 1932 and the first term of 1933 when there were in all twenty-three cases of mild but unmistakable diphtheria. This was somewhat of a shock to Dudley, who had been critical of similar outbreaks reported by others in immunized communities. In a sense, however, it was a fortunate occurrence, because it launched him on a series of investigations which he might not otherwise have undertaken. On making a detailed examination of the strains of diphtheria bacilli isolated, he found that all but two were of the *gravis* type, which at this time made its first appearance in the school. However, this more invasive strain was not necessarily the sole cause of the breakdown, for a series of Schick tests revealed a considerable number of 'relapses in immunity'—i.e. reversion from Schick-negative to Schick-positive. Until this time Dudley had accepted the view, then held in most quarters, that immunity, whether acquired naturally or artificially, was of long duration. He soon found, by making the necessary Schick tests on boys whose previous records were in his possession, that this was not so, and that both naturally and artificially acquired immunity tended, in a percentage of cases, to fall quickly below the arbitrary level set by the Schick test. However, the immune state—i.e. the Schick-negative state—was soon restored by relatively minor stimuli, such as, for example, the small dose of toxin used in the Schick test. Furthermore, he observed that 'relapsed' immunes had in fact considerable resistance, and an attack of clinical diphtheria in such cases was never severe. He thus was able to postulate a permanent basal immunity, a mechanism capable of responding rapidly to stimulation by the specific antigen of the diphtheria toxin. He also showed that the immunity enjoyed by those who are Schick-negative is antitoxic and not antibacterial. Thus, Schick-negatives can be carriers of virulent diphtheria bacilli, and when infection is rife usually show a considerably higher carrier rate than Schick-positives. The obvious but extremely important conclusion to be drawn from this is that, during an outbreak, active immunization will yield satisfactory results only if the whole 'herd' is immunized. (The influence of 'herd' environment, both in this and other matters, was of great interest to Dudley, and he expounded on it at considerable length in several of his papers.) His observations enabled him to conclude that, while active immunization is not infallible, if widely adopted its effect is likely to be cumulative.

In 1940 a nation-wide campaign of active immunization against diphtheria, which has brought about the virtual extermination of this disease, was

initiated in Britain. The decision to undertake this campaign was based on the results reported by many different investigators, among whom Dudley occupied a leading role. Although he dealt in relatively small numbers, the logical reasoning which directed his attention to certain specific points, the care with which his experiments were carried out, and the sound common-sense behind the conclusions he reached, gave to his findings a high degree of authority.

Epidemiology in general

Although Dudley devoted more time to diphtheria than to any other specific infectious disease, his interests were by no means confined to this particular condition, and it is clear that he hoped in his work on diphtheria to uncover basic principles of general application. In a selected but semi-isolated community of fit young men, such as is found in units of the Royal Navy, infectious diseases, and in particular minor infectious diseases, are the commonest cause of loss of time through sickness, and the problem of reducing this wastage was one which appealed strongly to him. He therefore had constantly under review both the factors which facilitated the spread of infectious disease and those which militated against such spread.

He came to regard specific resistance as the most important limiting factor. He postulated that this resistance, which may be of any magnitude or duration, depends on the nature and amount of previous contact with the proteins of the parasite concerned. Such resistance, distributed throughout the population, constitutes 'herd immunity', the ratio of those having resistance above a certain magnitude—immunes—to those lacking this resistance—susceptibles—being the 'herd immunity index'. He found that the spread of epidemics in space and time is a function of this index, combined with the density and movements of the herd. Unless the mass of the infective agent is sufficient to overcome this resistance, the parasite is destroyed by the host, whose resistance is thereby altered in degree. Time spent in an infective environment therefore plays a significant part in determining the resistance of the host. Dudley postulated a principle which he designated the 'velocity of infection', resulting from the interplay of the velocity at which the infective material is received by the host, the velocity at which it is destroyed by the host, and the velocity at which the host resistance alters in a positive or negative direction. On this 'velocity of infection' depends the final result of the reaction between the host and the invading organism. These and other hypotheses are supported by a mass of statistics drawn from a study of the incidence of sore throat, diphtheria, influenza, mumps, scarlet fever, rubella, and cerebro-spinal fever in varied communities such as the London Foundling Hospital, naval training establishments, and sea-going ships. In analyzing these figures, and in making his deductions therefrom, Dudley found full scope for his mathematical talent and for his lucid reasoning. The final dictum, made in his monograph on the spread of droplet infection in semi-isolated communities (1936) that 'the chief practical lesson . . . is that the individuals

of a community should be isolated from each other to the greatest possible extent in their sleeping quarters. Especially is this the case in those communities to which many susceptibles are frequently added', is perhaps somewhat of an anticlimax, but shows a refreshing degree of realism.

Dudley's advocacy of the philosophical approach to the problems of epidemiology became even stronger in his later years, as can be seen in the titles of his two Presidential Addresses to the Section of Epidemiology and State Medicine of the Royal Society of Medicine—the first, in 1935, 'On the biological approach to the study of epidemiology' and the second, in 1936, 'The ecological outlook on epidemiology'. His basic theme throughout was the establishment of active immunity by latent infection, and his pre-occupation a study of the factors, short of a clinical attack of the disease in question, which operated for or against the acquisition of this immunity. His breadth of vision and wisdom in these matters attracted attention far beyond the confines of his own service, and the principles he enunciated have found general acceptance.

Other scientific investigations

Dudley was fundamentally versatile, and was prepared to turn his attention to any subject which had a bearing on his central theme—the preservation of health in the personnel of the Royal Navy. Thus we find papers on the use of the kata-thermometer in the tropics, atmospheric hazards of one kind and another in ships, mass radiography, the danger of introducing yellow fever into India, and various other subjects, unrelated to each other except that they fall within the province of the naval sanitarian. The volume and scope of his writings give ample evidence of the opportunities which present themselves to the Service medical officer who is gifted with the ability to see and to grasp them.

ADMINISTRATIVE APPOINTMENTS

It is rare for a scientist in any of the medical services to escape administrative appointments in his later years—appointments which leave him little if any time to devote to his specialty—and Dudley was no exception. He was made Deputy Medical Director-General in 1936, and in 1941 became Director-General, an appointment which he held until January 1946. It is a very remarkable fact that his colleagues at this time, the Director-General of the Army Medical Service and the Director-General of the Royal Air Force Medical Service, had both, like himself, been pathologists in their earlier years. There can be no doubt that their mutual interests and expert appreciation of the practical problems involved in the preservation of health in the services did much to ensure the acceptance of the principles which they jointly advocated.

Dudley's responsibilities as Medical Director-General were vast and varied. Although he had to plan the massive expansion of the Naval Medical Service

rendered necessary by the world-wide commitments of the Royal Navy and to make all arrangements for attending to the sick and wounded, his main interest was in the preservation of the health of naval personnel, and in this he was able to make practical application of his earlier studies. He held that the only way of ensuring the adoption of hygienic measures was to explain them fully to the commanding officers of formations and others in authority, and thereafter to place the responsibility for implementing the instructions squarely on their shoulders. In carrying out this policy, his scientific reputation stood him in good stead, for he was much respected by his 'lay' colleagues, and was able to place his point of view before them in a forceful and convincing fashion. In his personal record he recalls with some glee an interview he had with Lord Louis Mountbatten, newly appointed Supreme Commander of South East Asia Command, whom he lectured on various basic principles of hygiene and in particular on the prevention of malaria, insisting that Mountbatten must appoint an antimalarial medical staff with the right of direct personal access to himself. Although Dudley was 'rebuked' for not having approached Mountbatten through the normal channels, he regarded this as being of little moment in comparison with the fact that his advice was adopted and his suggestions expeditiously carried out.

One of his most important achievements at this time was to persuade the Board of Admiralty to found the Naval Personnel Research Committee, with the Secretary of the Medical Research Council as Chairman, and, as members, non-medical naval officers in equal numbers with medical officers and civilian experts. The composition of this Committee, which Dudley arranged with the help of his friend, Sir Edward Mellanby, achieved his object. Previously, novel or revolutionary proposals put forward by the Medical Branch of the Royal Navy on its own authority were apt to be treated by the Lords of the Admiralty with the suspicion which the layman very often entertains towards such innovations. Coming from a predominantly civilian committee and with the concurrence of high-ranking non-medical naval officers, the position was very different, and proposals were accepted without delay or demur. This Committee still functions, and has been responsible for much good work.

Another achievement was the introduction of mass radiography in the Royal Navy. Dudley had long been interested in the possibilities of detecting at an early stage tubercular lesions in the lung, a condition of much potential danger when men have to live together in the close proximity which is unavoidable under Service conditions. His enthusiasm led to the development of this technique with such success that it was used in the Navy before it was adopted by any of the other services or on a national scale.

Although his colleagues suggested, somewhat ruefully, that Dudley placed medical administration on a much lower level than epidemiology, he was none the less a good administrator. He remained calm when chaos reigned, and in trying circumstances his decisions were sound, lucid and logical. The extent to which his ability was appreciated by his seniors is revealed by the

invitation given to him to extend the statutory period of his appointment and remain in office until the end of the war.

PERSONALITY AND PHILOSOPHY

Despite the fact that Dudley was endowed beyond the average with natural gifts, both physical and intellectual—and was not unaware of the fact—he had nevertheless an unusually shy and reserved disposition, so that in strange company he was monosyllabic and appeared to be aloof. When he was with friends, however, the shyness disappeared, and he talked freely and entertainingly, especially when the conversation turned in any way to work in which he was interested. In scientific meetings and committees he often showed the same aloofness, and in general he was much less apt with his tongue than with his pen. His silence was not an indication of lack of interest or of wandering attention, and his colleagues have remarked on his ability to write a brief yet brilliant summary with most pertinent comments on a discussion in which he took little or no part and in which he appeared to show little interest. As a young man he at first exhibited the same diffidence regarding his scientific work, but fortunately he was brought in contact with others who shared his interests and disagreed with his modesty. In the early 20's he was invited by Sir Paul Fildes (who was pathologist at Haslar during the first war, and there made Dudley's acquaintance) to read a paper before the Medical Research Club. There he came to the notice of Bullock, Topley, and Major Greenwood, who were quick to recognize the importance of his findings and who encouraged him to continue his research. The friendships thus started were life-long, and Dudley writes that he owes more than he can acknowledge to the guidance and help he received from these three men. Although he worked harmoniously with his naval colleagues and was much liked by them, Dudley was essentially an individualist. He considered himself too independant and lazy (this, however, sounds like one of the so-called 'whimsies' which he describes when he writes about psychology) to work under direction, and too diffident to direct a team of research workers, and preferred as far as possible to follow his own bent without direct assistance from others. His success as an investigator was probably due to his keenness and persistence, his capacity for taking pains, and the scrupulous honesty he exercised in interpreting his findings. His mathematical brain stood him in good stead in epidemiological work, though a critic might say that sometimes his enthusiasm outran his judgment. On more than one occasion he went to great trouble to construct a complex mathematical equation for the solution of a problem and then, as far as can be seen, passed on without using it in any practical way.

In his years of retirement Dudley wrote two books, *The four pillars of wisdom* and *Our national ill health service*. Of the second of these, little need be said. It is not so much an indictment of the existing National Health Service as a plea that more effort should be devoted to preserving the health of the healthy

than to succouring the sick. *The four pillars of wisdom* is of greater interest in that much of it is autobiographical, an analysis of Dudley's own philosophy. The four 'pillars' are semantics (the science of meanings), psychology, statistics and logic—in the last paragraph of the epilogue of the book he gives, as synonyms, perspicuity, human nature, a sense of proportion, and common-sense. In advocating the inclusion of these subjects in scientific curricula, he draws from his own experience to illustrate the disadvantages under which he laboured until he had, through his own efforts, acquired an adequate knowledge of them, and enlarges on the importance of having a fundamental grasp of these subjects in order to be able, in the course of scientific investigations, to separate facts from fancies. A chapter on religion and psychology tells much of the doubts and conflicts of loyalty which troubled his early years. Brought up in a strict religious atmosphere, he found himself unable to accept the teaching of his parents and instructors; and, having while still a schoolboy studied Darwin, Haeckel, Spencer, and Grant Allen, he turned to agnosticism with, he says, great mental relief and freedom, and was able 'to become a normal member of the herd'. It is clear, however, that he never did become a normal member of the herd, and throughout his life the sceptical yet questing outlook which he evinced when breaking away from the religious teaching of his early years remained an integral part of his personality. He attributed great importance to 'herd instinct' and 'herd fashion' and was gravely suspicious of the intrusion of emotion into scientific argument and reasoning. Nevertheless he retained his sense of proportion. 'Nothing is more irrational', he wrote, 'than to be always pedantically rational. But it would make the world easier to live in if people could truly believe that emotional values were not to be placed in the same category as objective facts'.

Dudley's father feared that his son would waste his life if he entered the medical service of the Royal Navy. In 1950 the son was able to write—'I can truly say that I would join the Royal Navy again and as a doctor, if I had my time over again. I have had a splendid life, playing, working, and lazing all over the world, meeting all sorts and conditions of men, and being a member of the finest herd in the world, the Royal Navy.'

J. S. K. BOYD

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L. P. Gurnea

LEWIS LEIGH FERMOR

1880-1954

LEWIS LEIGH FERMOR died on 24 May 1954, at his home in Horsell, Surrey. He was born on 18 September 1880, the eldest of six children of Lewis Fermor and his wife Maria James. His father, a bank clerk in the London Joint Stock Bank, was obliged to retire prematurely due to ill health, and as a result the education of the family was a major problem. The young Fermor was taught by his mother up to the age of seven but was sent to the Goodrich Road Board School when the family moved to Dulwich. During the first term his education cost 4d. per week but by the following term elementary education had become free, and from this time his parents had nothing more to pay towards the education of their eldest son. From this school, to which Fermor always paid high tribute for the excellence of the teaching, he obtained a scholarship to Wilson's Grammar School, Camberwell, and here he was fortunate in having the late Sir Percy Nunn as science master. It was largely due to the encouragement given by Nunn and Sir Thomas Kirke Rose (a cousin of Fermor who was at that time Chemist and Assayer at the Mint) that Fermor decided to try for a National Scholarship to the Royal College of Science and thereby to enter the Royal School of Mines, and work for an Associateship in Metallurgy. The competition for these scholarships was severe, and Nunn warned him that he would have little chance of success unless he was prepared to undertake a special course of additional reading which would have to last two years. A scheme was drawn up whereby he was to rise at 5 o'clock each morning, take a cold bath, do two hours' work before breakfast, work another two hours in the evening, and always be in bed by 9.30 p.m. In his later years in some autobiographical notes which he wrote for the interest and amusement of his wife, he mentions that there was no hardship in this. The hardship was in the summer, turning into bed whilst the brothers and sisters were still out in the garden. Fermor kept to this régime loyally and was rewarded for his perseverance by obtaining his scholarship and entering the Royal School of Mines in October 1898. Here he obtained a first class in each year's course and won the Murchison Medal for Geology, a prize of books to the value of £15, and secured his Associateship of the Royal School of Mines in metallurgy.

In the summer holidays of 1900, after the second year's examinations were over, he started studying for the London University Matriculation in order to work for a degree. Latin was then a compulsory subject, but he knew none as he had chosen to take science at school. He borrowed a Latin grammar

and proceeded to teach himself by working assiduously through it. The work set for the examination was Virgil Book V. When the results of the Matriculations were published he was second in the entire list of 1500 candidates, winning also an Exhibition that he was debarred from taking as, at twenty, he was too old.

He intended to continue with metallurgy but was persuaded by Professor Judd to apply for a post in the Geological Survey of India, for which there were then two vacancies. He was interviewed by Dr W. T. Blanford, F.R.S., a retired officer of the Department, and shortly afterwards was offered and accepted the post of Assistant Superintendent at a starting pay of Rs.350 per month (£315 per annum). Fermor was then more a chemist and a metallurgist than a geologist, but Blanford's judgement in selecting him for the Survey was thoroughly vindicated. By the end of the 19th century much reconnaissance mapping had already been undertaken of the Archaean formation of India, but relatively little of the country had been studied by the then recently developed techniques of petrology. Blanford, one of the greatest of Indian naturalists, would have seen in Fermor's chemical and metallurgical training the requisite background for a new approach to the crystalline rocks. This proved, indeed, to be the principal work to which Fermor became dedicated. The late Dr G. E. Pilgrim, F.R.S., was appointed to the other vacancy, and the two young scientists went to India together, landing in Calcutta in October 1902.

One of the first tasks allotted to Fermor by his chief, Sir Thomas Holland, was the investigation of the manganese deposits of India. Indian manganese ores were first worked for export in the Vizagapatam district of Madras, now Andhra State, in 1892. The success of this venture had encouraged widespread prospecting, and by 1902 there was an export trade not only from Madras but from the Central Provinces and Bombay. The Geological Survey had, however, no information about the nature and extent of the ore deposits, and Fermor was sent to examine them. This he did with his usual patience, vigour and determination. From 1904 onward he published numerous papers on different aspects of the Indian manganese deposits, and obtained successively the B.Sc. and D.Sc. degrees in London University for this work. These early papers were all incorporated in 1909 in his classic *Memoir on the manganese-ore deposits of India*, a monumental work running to over 1200 pages, profusely illustrated with maps, diagrams and photographs.

The *Memoir* opens with a general discussion of the world's manganese deposits, followed by a full description of the known manganese minerals. It is given to few geologists to find new minerals, but Fermor described Hollandite, Vredenburgite, Sitaparite, and Khoharite, which he regarded as new species, and Juddite, Blanfordite, and Winchite which he regarded as hitherto undescribed varieties of known species. Subsequent work with the ore microscope and X-ray techniques has shown that some of these are not unique mineral species. Vredenburgite, for example, is an ex-solution intergrowth of Jacobsite and Hausmannite which only seldom occurs as a

homogeneous metastable mixture. The isolation and recognition of these mineral species and mixtures by Fermor, long before the development of techniques which permitted the opaque minerals to be studied in greater detail, was an important advance in providing data about phase stability under varying conditions of mineral paragenesis.

The geology of the Indian manganese-ore deposits is then discussed in detail, and the limitation of the workable deposits to the Archaean complex is noted, the most important being associated with either the Kodurite series of Vizagapatam District or the Gondite series of Madhya Pradesh (Central Provinces), Bombay, and Orissa.

The Kodurites form a series of deeply decayed manganiferous rocks rich in potash and phosphorus, the origin of which is still doubtful because of the paucity of unaltered minerals and the poorly developed contact relations. Enough exposures do occur, however, to show that the primary minerals are orthoclase, manganiferous garnet and apatite, which give the rocks a decidedly igneous appearance. By extrapolating in cases where the end products contain no relics of original minerals, and by making certain analogies with the contact relationships of intruded Gondites in Madhya Pradesh, Fermor was led to regard the Kodurites as representing a hybrid product formed by the wholesale incorporation of manganiferous sediments by invading granitic magma. The modern manganese ores derived from the Kodurites are believed to have been formed in these hybrid rocks through the action of carbon dioxide carried by meteoric waters. The whole picture, based as it is mainly on deeply decayed material, is still controversial, but so far no one has been able to produce an alternative interpretation.

In Madhya Pradesh he claimed that in Archaean times there was a series of lakes in which were deposited both chemical and mechanical sediments. The chemical deposits were rich in manganese, derived possibly from Kodurites or other manganese-bearing igneous rocks. They were followed by a series of deposits partly chemical and partly mechanical, in which manganese was also present, and finally by a thick series of sandstones, clays, and iron-ores. The whole complex was intensely folded at the close of the Dharwar period. Many of the chemical sediments were at this stage converted into rich manganese ores composed of crystalline Braunitz, and the sedimentary succession became much denser than it had been before folding. Under the influence of the high pressure the mixed chemical and mechanical sediments were converted into banded spessartite-rhodonite-quartz rocks which Fermor called Gondites. These were unstable, and in the course of time manganese replaced the silica both in spessartite and rhodonite, forming what he describes as secondary Archaean ores. Where there was little metamorphism, as in the Chilpi Ghat series, only primary ores are found.

Other manganese ores of economic importance seen by Fermor are lateritoid in nature. These have been formed by surface replacement of the original phyllites. Sometimes, as in Sandur State, in Jabalpur district, and Orissa, this surface replacement, which extends to a depth of 50 feet or more, contains

both manganese ores and manganiferous iron ores. This relationship between distribution of secondary manganese ores and meteoric water, under topographical control from earlier cycles of erosion, is now well established in India and as far away as Brazil, and such deposits represent an important source of ore, even though the quality is not as good as that of the primary Gondite ores of Madhya Pradesh and Bombay.

In Part III, Fermor gives an interesting historical sketch of the beginnings of the manganese industry, a very important discussion on the structure of the deposits, many of which occur in pitching folds, and an account of mining and quarrying methods with advice on opening up new deposits. The last two items have proved of great value to ore-mining companies.

He estimated the life of the manganese deposits as only some 20 to 30 years, allowing for an average working rate of 400000 tons a year. This has proved to be a conservative estimate in the light of subsequent work and of his own paper on the age and continuation in depth of the manganese ores of the Nagpur-Balaghat areas, for he there concludes that the alteration of the Gondite series to manganese ore took place to a great depth in Archaean times. The main uncertainty was in fact in regard to the lateral extent of the individual deposits, and a more definite estimation of reserves had to await intensive large-scale mapping. This work was not undertaken until 1950 when three parties, working under the direction of Mr Straczek, seconded from the United States Geological Survey, started a systematic study of the Balaghat area, Madhya Pradesh, with the aid of air photographs. The syngenetic origin of these deposits, first indicated by Fermor, was confirmed. The deposits were found to be lenticular, ranging in length from 100 to 10000 feet, and at Tirodi they have been proved to occur in the form of a recumbent fold. Estimation of quantities has been difficult on account of lenticularity and folding, but, in the light of the detailed work which has now been completed, aided by more extensive exposures and data from the mines, a single mine such as that at Mansar is thought to possess reserves of the order of 2 to 3 million tons.

In Part IV of his *Memoir* Fermor describes in considerable detail all the then known manganese deposits of India. This has been of great value as a reference volume and is a witness of the thoroughness with which the studies had been made almost half a century ago. The whole *Memoir*, which is still a standard and authoritative text, was the work of a man yet in his twenties.

In addition to his study of manganese ores, Fermor undertook during the first eight years of his service investigations on chromite deposits in Baluchistan and Singhbhum, copper ores in Sikkim, bauxite in Balaghat, and in Satara, Bombay. He also acted as Curator of the geological galleries of the Indian Museum from 1905 to 1907, in which position he found time to write up numerous notes on the field work of his earlier years, on meteorites, and on the crystallography of some of his mineral finds. He wrote a note on the lavas of the Pavagad Hill which may be regarded as the beginning of his long interest in the Deccan Trap.

An important result of Fermor's work on manganese ores was that it gave him an opportunity to see the Archaean rocks of India in many areas. From this arose his early determination to investigate and decipher the relationship of the various Archaean systems in India, but it was not until many years later that he was able to begin setting forth his views in 'An attempt at correlation of the ancient schistose formations of Peninsular India', to be described later.

In 1910 his outstanding work was rewarded by promotion to Superintending Geologist, and from 1911 onwards administrative and war duties considerably interfered with his scientific pursuits. Nevertheless he continued to publish frequent papers on manganese and other scientific subjects.

Among these his 'Preliminary note on garnet as a geological barometer' and 'On an infra-plutonic zone in the earth's crust' published in 1913 is perhaps his most original paper, and won for him the Bigsby Medal of the Geological Society, London. In this paper Fermor makes a case for an infra-plutonic zone composed largely of garnet but with some granitic patches. He shows that at high pressures pyroxenes, olivines, and iron ores, all tend to be converted into garnet with a consequent reduction of volume of 20 per cent. Alkalis not required in this process, together with surplus alumina, silica, and ferric oxide, were considered to form garnetiferous granites. He built up a picture of the earth's structure in which light-weight continents float on a basaltic zone that passes at depth into an infra-plutonic zone composed mainly of eclogite. Beneath this zone he visualized one of peridotite, or mixed peridotite and nickel-iron, resting on a nickel-iron core. The achondrite meteorites correspond to his basaltic zone ; the chondrites to his infra-plutonic zone, whereas the mixed peridotite and nickel-iron types are regarded as equivalent to his deepest zones. The floating of the continents was accepted as proved, but was thought to be possible only when the infra-plutonic zone had changed phase from solid to liquid by the accumulation of radio-active heat. He attributed deep-focus earthquakes to sudden explosive changes of phase in the infra-plutonic zone, and looked to the same cause to provide sufficient superheat in the plutonic zone to account for the enormous outpourings of basalt such as exist over the Deccan plateau. The migration of continents was considered to have post-dated the jettisoning of the moon by the earth.

Some of Fermor's work on garnets has subsequently been modified by the development of X-ray analysis, and the theory that chondrites were once garnets has not found wide acceptance. The chief value of these speculations has been the emphasis placed on the effect of pressure in depth on mineral transformations.

In 1914 and 1915 Fermor continued his work in the Central Provinces (Madhya Pradesh) and published in collaboration with C. S. Fox an interesting paper on the Deccan Trap flows of Linga, Chhindwara District. The surveys of Blanford in the Purna Valley had previously shown that the traps are locally folded, but the mapping of Fermor and Fox was the first

detailed study of the disposition of the lava flows east of the Western Ghats, and demonstrated that the traps were both folded and faulted, and that the folds followed a pre-existing Archaean trend. The flows were considered to have travelled over a distance of at least 25 miles.

During this time Fermor investigated the coal resources of Korea State in Madhya Pradesh, in wild forest country, sparsely inhabited, with no road and but a few forest tracks. The Raja of a neighbouring state allowed few of his subjects to carry firearms as he preserved the local tigers for his own 'shikar'. Fermor was provided by the Raja with a small bodyguard of local aboriginals armed with bows and arrows. This guard was certainly needed, as later workers in the area amply confirmed. It is interesting to see the brilliant lights of a flourishing mining centre in the jungle where Fermor mapped several of the valuable coal seams of the Korea Field.

It is generally difficult to distinguish good-quality coal from high-ash coal by eye, so he evolved a rough and ready method for field use by which the ash content of the coal could be calculated from its specific gravity as measured on a Walker balance. This method has been of value to geologists in the field since, after calibration by proximate analyses over the full range of ash content, specific gravity determination obviates much analytical study, and affords an immediate indication of the merits of different seams. Between 1928 and 1930 several papers were published dealing not only with the density-ash relationship, but with the colloidal nature of coal substance.

Fermor's services were later lent to the Railway Board, for whom he wrote reports on the Karanpura and Talcher coal-fields, and mapped a portion of the Bokara coal-field where he proved the presence of 730 million tons of coal over an area of 10 square miles in the Kargali seam. He impressed the Government of India with the necessity of avoiding waste of the better quality coals in all the Indian coalfields. The advice tendered by him ultimately led to legislation designed to encourage sand-stowing and to reduce the use of coking coal (needed for smelting India's enormous iron-ore resources) in locomotives and factories which could use non-coking coals.

While mapping at Bokara he observed rocks of igneous appearance similar to those named by him Chirmiri Volcanic series in Korea State. He was able to prove by shallow drilling that such rocks in Bokaro are the burnt outcrops of coal seams which are undamaged when followed below the water level.

In a paper published in 1928 he contrasts the behaviour of the Indian coal seams in contact with basalt with that of a similar contact which he had seen in Skye. The different behaviour of the seams in these two localities is accounted for by the fact that the Indian coals are regarded as dry while that at Skye had probably been at the bottom of a lake.

In 1917-1918 he acted as Mineral Adviser to the Indian Munitions Board. Here he worked out schemes for manufacturing calcium carbide and nitrogenous fertilizers in India. He also investigated the possibility of smelting aluminium-ore in India as a war measure. At the end of the war he was awarded the O.B.E. for his work on these two boards.

After the war Fermor returned to his position in charge of the systematic mapping in Madhya Pradesh, and spent his available time in Sausar Tahsil, Chhindwara District, undertaking the first detailed mapping of the Indian Archaeans, with a precise regard to strike continuation and mineralogical change, in a manner comparable to the classic researches in Canada, Finland and Scotland. Field work of high precision was accompanied by studies of thin sections under the microscope, the results being plotted nightly on a 4-inch-to-one-mile field map. This routine continued intermittently for thirteen years and, although the results appeared annually in the *General Reports of the Geological Survey of India*, pressure of other duties prevented any description of this work as a whole. The map was in fact never published until a part of it was reproduced as a plate on page 176 of Volume I of the third edition of the *Manual of the geology of India and Burma* prepared by Sir Edwin Pascoe (1950).

In addition to this field work Fermor had many other activities during the post-war period. In 1919 he published an interesting note on some problems of ore genesis of the Archaeans of India. In this he points out the comparative abundance of syngenetic ore deposits such as manganese and iron, and the rarity of epigenetic deposits like gold and tin. He thought it possible that most Indian granites and gneisses are secondary, due to the metamorphism of older rocks, and only a few are primary and therefore likely to be the source of epigenetic ores.

In 1924 he published an important paper on the 'Pitch of rock folds', pointing out from practical experience in Madhya Pradesh the necessity of taking 'pitch' into account in mining Archaean manganese ores.

Following up his interest in the lava flows of Linga, a detailed study was made in 1925 of the basaltic lavas from the Bhusaval boring, Bombay. In this he courageously overhauled the nomenclature of the various glassy substances so common in the Deccan Trap. He defined palagonite, delessite and chlorophaeite, generally introducing order where there had previously been confusion.

He grouped the large number of flows which were penetrated by the boring firstly by the presence or absence of felspar phenocrysts and olivine, secondly according to whether the olivine was altered to iddingsite or serpentine, and thirdly by the occurrence of iron ore in bars, or grains. The flows were divided into two main types : magma type I in which the iron ore occurs as grains, and magma type II where it occurs mainly as bars. He attributed minor differences arising from the sinking of felspar phenocrysts and olivine to variations in the fluidity of the magma with possible tapping at different levels. All zeolites, palagonite, opal and quartz were regarded as late magmatic developments which took place at a temperature of 270°C or lower. The alteration of olivine and the deposition of some calcite were also low-temperature changes of the same general period. The absence of inter-trappeans shows that the flows followed one another in quick succession. Nevertheless, the presence of thin red partings between the flows indicates



that they were separated by short intervals during which oxidization could take place, and in the Western Ghats of Satara District such partings are represented by mappable beds of red bole.

This microscopical work of Fermor was the precursor of studies undertaken later by younger colleagues, firstly of several borings in the Deccan lavas of Kathiawar (Saurashtra), and more recently in connexion with the systematic boring of dam sites and diversion-tunnel schemes located on the Konkan and high plateau areas north-east and south of Bombay. The observation that olivine and felspar phenocrysts sink in the Deccan Trap is certainly an important addition to our knowledge of that formation. Three more short papers on the 'Enstatite-augite series', on the 'Constitution of glauconite and celadonite', and on the 'Chemical composition of the Deccan Trap flows of Linga, Chindwara District, Central Provinces' complete Fermor's work on the Deccan Trap.

Fermor very early took an interest in the magnetic properties of manganese minerals such as Braunitz and, in using a Goolden's Portable Dip Circle, or vertical variometer, during the early part of this century in the field study of manganese ores, he was amongst the first exponents of geophysical methods in the search for minerals. Such procedures are now common practice, but nearly fifty years ago the idea was advanced and original.

Before leaving India Fermor began a great work in three parts entitled 'On an attempt at the correlation of the ancient schistose formations of Peninsular India'. The first volume, part 1, published in 1936, is an introduction explaining how this correlation could best be made, and classifying the Archaeans of India and Burma into 4 regions, and 18 provinces. The second published in 1938 consists of three chapters each describing one of these provinces. The third published in 1940 describes in one chapter a fourth province. It was Fermor's intention to complete part 2 by writing a chapter on each of the remaining provinces, and then in part 3 to make an attempt at correlating all the provinces. Unfortunately parts 2 and 3 were never completed.

The second volume gives detailed descriptions of the Dharwars from the type area in Mysore, and describes similar rocks in Bihar and Orissa and Bastar which he regards as all part of one great sedimentary system. The third gives Fermor's best general account of his detailed mapping in Madhya Pradesh. Obviously he would like to regard the rocks there as part of the same great system, and the calcareous rocks of the Sausar series are tentatively correlated with the calcareous facies of Dharwars at Sakarsanhalli. Nevertheless, these rocks are considered in Mysore to be older than those in the main Dharwar sequence and it is thought possible that they may even belong to an earlier sedimentary system. If this Mysore relationship does apply to Madhya Pradesh, the Sausar series would be older than the Balaghat Dharwars. Fermor did not however believe that this is so, since both Balaghat rocks and the Sausars contain manganese deposits and a remarkable quartzite with much ilmenite and rutile, characteristics which he considered as

sufficient evidence for contemporaneity. He accepted, however, that much detailed work is necessary in Balaghat before the correlation can be definitely established.

The work involved in a review of the Indian Archaeans was great. Much of the study of these rocks had been done before the invention of the petrological microscope and adoption of chemical criteria, and the rather vague qualitative descriptions of the pioneer geologists had to be examined and re-interpreted in the light of modern knowledge. This in itself was an undertaking which required great judgement and experience. It was even more difficult to re-write sections of the work where there had been a change in geological ideas since the turn of the century, as for example in the Sausar area. Geological investigations began there in 1904 and continued intermittently up to 1935, not being renewed until 1950 when, as stated earlier, a re-assessment of the manganese reserves necessitated intensive regional mapping. The Sausar rocks occur as calcareous and siliceous para-gneisses located in a granitic terrain. Originally the granitic material was regarded as intrusive granite, but later work has shown that much of it is sedimentary, associated with magma in an assimilation zone. How far this has upset the succession on the basis of earlier concepts is not known, but it is a good illustration of the modifications which inevitably arise when field work is spread over 30 years.

Fermor officiated as Director of the Geological Survey of India in 1921, 1925 and 1928, and finally became Director from 1930 to 1935. These periods were a time when the British rule in India was approaching its end ; it also included the great world slump. In these circumstances no great administrative change was possible and Fermor had the difficult and unpleasant task of defending the Geological Survey first before the Inchcape Committee, and secondly before the Government of India Retrenchment Advisory Committee of 1931. The former, headed by a business man was persuaded not to cut down the survey staff. The latter consisted of politicians and civil servants who wished to reduce the Geological Survey from thirty serving officers to eight, but Fermor pleaded the Survey's case with such ability that he succeeded in keeping twenty.

The civil servants of that time adopted a narrow and statistical attitude which thought of the staff and equipment of a scientific department as so many units of expenditure, regardless of function in the economy of the country and of the peculiar educational background necessary for the subsequent recruitment of scientific personnel when conditions improved. The Survey was already in 1926 committed to complete Indianization, and from then onwards recruits would come mainly from Indian Universities. Since there were few openings for graduates in commercial geology, the Geological Survey represented almost the sole avenue of employment. Retrenchment in the survey involved curtailment in the Geological departments, the effects of which were only later felt when expansion began with an independent government. Nor did the Retrenchment Committee appreciate

the intimate connexion between academic and applied geology in the study of economic resources such as in the determination of mineral and ground-water provinces, seismological studies, and the geological aspects of civil-engineering projects. It took the Survey many years to recover from the ill-conceived economies which were forced, against every protest, on Fermor in 1931. When, indeed, 19 years later an Indian Economy Committee studied the Geological Survey on a similar mission, it had the vision to recommend an increase in expenditure, notwithstanding the economies which had to be imposed on almost all the non-scientific departments. The recent policy bore a resemblance to the constructive attitude to long-term investigations manifested at the worst period of the 1929-1931 depression in the United States, when projects were initiated which have been of lasting value to the country.

In 1935 Fermor was made a Knight Bachelor, and was elected a Fellow of the Royal Society. He retired in the autumn of that year from his position as Director of the Geological Survey, but remained in India for another six months working on the Archaean Memoir. The year 1936-37 was spent in Kenya, Natal, and South Africa widening and enriching his experience in many branches of geology.

In 1938 he went to Malaya at the invitation of the Colonial Office, and wrote an extremely valuable report on the mining industry of that country. The main object of this report was to advise the Government on a sound policy of mineral exploitation, but this involved also making suggestions regarding the satisfactory relationships between the various departments concerned with different aspects of the industry, such as Geological Survey, Mines, Forests, Agriculture and Irrigation.

In his report Fermor examines the history of mining, particularly tin mining in Malaya, and shows with the help of numerous statistical tables how important this industry had been in the rapid development of that country. While admitting that certain forms of tin mining have led to extensive erosion and some damage done to river beds, he demonstrated that the harmful effects had been effectively dealt with by regulations for the disposal of tin residues and were in any case small compared with the erosion due to rubber planting and other forms of agriculture.

He deals with the effects of tin restriction, both good and bad. The worst result had been the almost complete stoppage of tin prospecting. Finally he recommends that prospecting of all minerals be pushed on rapidly until a complete picture of the resources of the country becomes available. This work would be done in part by the Government and in part by private enterprise. To undertake the Government's share in this work an increase both in the Geological Survey and the Mines department would be needed. Another important point which he stressed is that the whole revenue from minerals should not be spent, but that a portion of it should be paid into a sinking fund, the revenue from which would suffice to make up the loss of revenue to be expected as the easily worked mineral deposits become

increasingly exhausted. The tragedy is that Malaya was engulfed in a great world war before these recommendations could be put into operation, and conditions are now so changed that some of the report may lack its original relevance. The fact that the Japanese printed a special edition of this report indicates the high value they attached to it.

He published two other short papers on Malayan geology. One is on the varved Triassic sediments, the banding of which he considers was due to seasonal changes of climate. The other paper describes some interesting vertical veins of coal which were noticed both in the alluvium and in the schists of Malaya, and were regarded as clear evidence of the colloidal nature of bright coal (vitrain).

In addition to this Malayan Report he undertook, after his retirement, consulting work on behalf of various private interests, in the course of which he visited Egypt, Southern Rhodesia, Angola, Lisbon, and also revisited India and Malaya.

The foregoing account of Fermor's more important published papers, of which a full list is appended, is necessarily compressed. His largest single contribution has been the manganese Memoir, which has greatly advanced the scientific knowledge of the world's manganese ores, and has been a major contribution to their mineralogy and petrology. It has also been of great practical importance to miners and has given the Government of India an account of the nature and distribution of these deposits on which it was possible to base a rational policy for their economic extraction. Next in importance must be considered the three volumes on the correlation of the ancient schistose formations of Peninsular India, which have clarified the very difficult problems involved in correlating the various crystalline schists scattered over India, and laid a foundation for future research.

During his life in India, and again in England after his retirement, Fermor took an active part in many scientific and other societies. On four occasions, in 1910 (Sweden), 1915 (Canada), 1926 (Spain), and 1929 (South Africa), he represented the Government of India at sessions of the International Geological Congress. He was a founder member of the Mining and Geological Institute of India (now the Mining, Geological and Metallurgical Institute of India), and contributed many papers for publication in their *Transactions*, one of which on 'Manganese in India', was awarded the Government prize and medal. For many years he was their Editor and Honorary Secretary, and in 1922 became President. Later he was made an honorary member of the Institute.

In 1906 he became a Fellow of the Asiatic Society of Bengal and took a keen interest in its work. In 1933 he became its President and by his patience and tact did much to restrain the rival factions which at that time were making the work of this ancient society difficult. In 1944 he was the first recipient of the P. N. Bose medal presented by that Society for contributions to the geological knowledge of Asia.

In the early 1930's a large number of scientific societies existed in India, and it became obvious that some sort of central society was necessary to direct and correlate their work. To meet this need Fermor took a leading part in founding the National Institute of Sciences of India, and became its first President. This Institute has the same position in India as the Royal Society has in England. Without Fermor's persuasiveness, and good temper, it is doubtful if this important development in Indian science could have been brought about. His disinterested work in connexion with these two societies deserves the highest praise.

He was Vice-President of the Society of Economic Geologists in 1932; of the Institute of Mining and Metallurgy 1942-45; and President in 1951; Vice-President of the Mineralogical Society 1943-46 and of the Geological Society of London 1945-47. He was a founder-member of the Society of Economic Geologists and an Assistant Editor for *Economic Geology* 1923-37. In 1933 he was President of the Indian Science Congress, the Indian equivalent of the British Association. The Société Géologique de France made him Correspondent Étranger in 1948.

Societies whose geological affinities were less marked were the Himalayan Club of India of which he was Vice-President in 1931 and 1932, and the Bristol Naturalists Society of which he was President in 1945-47.

Fermor's interests were wide and included as well as geological subjects, natural history (particularly botany and butterflies) and philately. He was well informed on such subjects as Persian rugs and old English glass, considerable collections of which he had made.

With all these activities he was always the same kindly approachable adviser to small societies and to individuals. Untrained collectors of mineral samples from the lesser known parts of Orissa and elsewhere would bring to his Calcutta office sackfuls of miscellaneous specimens to be named. Some of these ultimately proved to be of great economic importance.

In his work in the jungle he spoke the aboriginal tongues and, as any scientist would, studied the local customs and culture. But few men could retain in memory, as he did, a highly inflected aboriginal language with no opportunity to use it for many years. He worked in Singhbhum in the early years of his Indian service; he returned to the area over twenty years later, in 1930, on a visit to one of the iron mines, and conversed with the local villagers in the Ho language, to their mutual delight.

In 1909 he married Muriel, daughter of Charles Ambler of Dharhara, by whom he had a son and daughter, the former, Patrick, now becoming well known as an author. He married again in 1933, Frances Mary, daughter of the late Edward Robert Case of Fiddington, Somerset, who survives him.

His staff found him a thoughtful and wise administrator and he was conspicuously fair in his dealings with everyone, minutely painstaking in his work, profoundly devoted to the science he chose as his profession, exceptionally clear and logical in the enunciation of any subject and with a capacity for work far beyond the average. His life was indeed a most successful

blending of academic, economic and administrative capacities. Academic studies are often later found to have important practical applications, a fact which is recognized by all sound Government Geological Surveys responsible for delineating the regional structural framework of a country. There have, however, been periods in the histories of geological surveys when the economic advancement is rather overlooked in attempting to achieve the background knowledge. Sir Thomas Holland woke the Geological Survey of India to its larger responsibilities, and Fermor, who was appointed under Holland, while by temperament a research geologist, always kept in mind the wider field of economic application which is the ultimate function of a state survey as distinct from a university. In his career there is the work on the Archaean foundation of India, the work on manganese and coal, and the speculative considerations, together exemplifying the proper activity of the Department he loved so much and served so well. Advancing years did nothing towards damping his enthusiasm or curtailing his industry. His death at a time when all arrangements had been made to complete his work on the correlation of the Indian Archaeans is a permanent loss both to Indian and world science.

H. CROOKSHANK
J. B. AUDEN

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Sir Lewis Fermor's publications, consisting of some 170 memoirs, addresses, papers and notes have appeared in the publications of the Geological Survey of India, the Mining and Geological Institute of India, the Asiatic Society of Bengal, the National Institute of Sciences of India, the Iron and Steel Institute, in the *Geological Magazine*, the *Mineralogical Magazine*, and elsewhere; and relate to geological, petrological, mineralogical problems, to meteorites, economic geology, mineral statistics, and to general scientific problems. A selection of the more important of these is given below.

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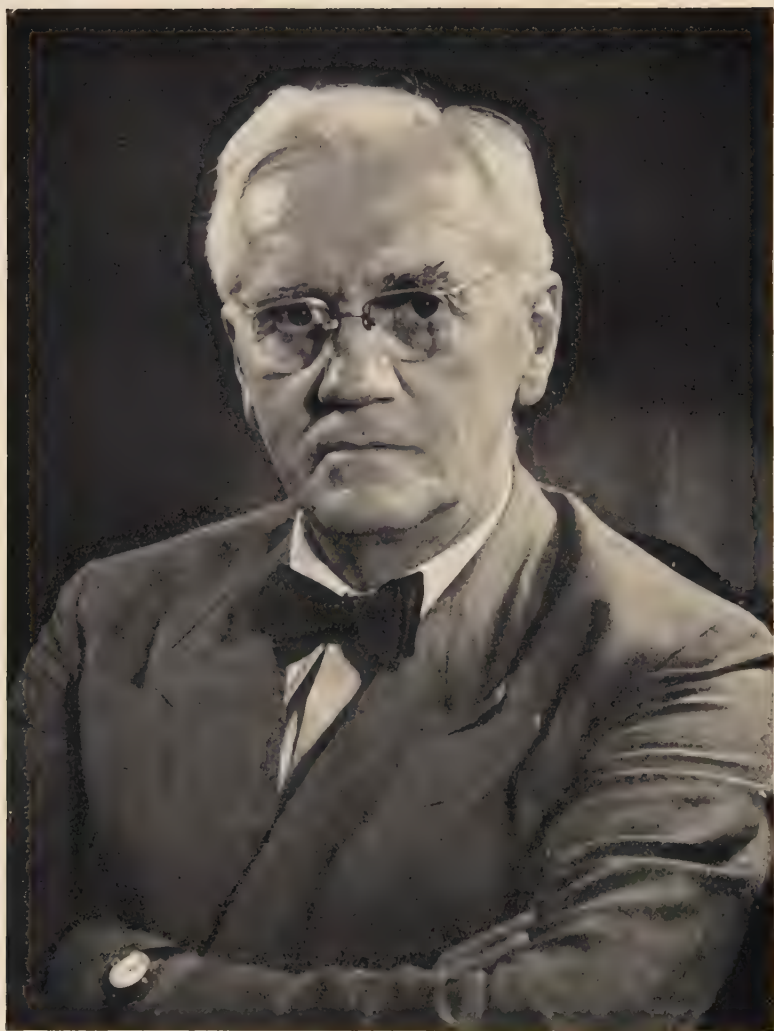
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Alexander Fleming

ALEXANDER FLEMING

1881-1955

ALEXANDER FLEMING was the youngest of four children born to an Ayrshire farmer, Hugh Fleming, by his second wife Grace (*née* Morton) on 6 August 1881. His education, up to the age of twelve, was at the village school (Darvel) and, for a further two years, at the Kilmarnock Academy. At fourteen he joined his brothers in London, where he worked for a time as a clerk in a shipping office, and also attended some classes at the Regent Street Polytechnic.

His eldest brother Thomas was already practising as an ophthalmologist and, perhaps influenced by his example, Alexander also decided to study medicine. His choice of a medical school (which was to prove all important) seems to have been largely determined by the fact that he had competed in a water polo match against the students of St Mary's Hospital. He therefore sat for an Entrance Scholarship at that Medical School, and won it, in 1901. After winning many other prizes as a student, he duly obtained his London University degree, M.B., B.S., in 1906.

At that time he seems to have had no strong predilection towards any particular sphere of medical practice. Surgical work evidently made some appeal to him for he proceeded to take his F.R.C.S. examination, and it may well be that, if he had pursued that career, his great technical ability and high intelligence would have made him an outstanding surgeon, and won him a substantial fortune.

But another road opened for him—in 'laboratory medicine'. As a student he had come under the influence of Almroth Wright, who had come to St Mary's in 1902, nominally as Professor of Pathology, but actually with the intention of developing a new road in medicine—that of therapeutic immunization against infectious disease. Wright had already, while serving in the Army Medical Service, successfully launched preventive immunization against typhoid fever. The new proposal went further. It aimed at stimulating the patient already suffering from a bacterial infection to make an immediate response to that infection by the elaboration of 'antibodies'; and it sought to follow that process by measurement of these antibodies in the patient's blood. This called for new techniques and considerable labour. The group of young men who had joined Wright, including John Freeman, Bernard Spilsbury and John Wells, were already unable to cope with it. So it happened that Fleming was invited to join the team as soon as he obtained his degree in 1906. (The choice fell on him partly because he was a good rifle shot and his

retention at St Mary's would help the hospital's team to compete successfully at Bisley.)

Having thus found his way into the first research laboratory to be attached to a hospital in this country, Fleming remained there until his death 50 years later, having ultimately, in 1946, succeeded Sir Almroth Wright as Director of a greatly enlarged Institute, and having himself become world-famous as the discoverer of penicillin.

During those 50 years he made many notable contributions to medical progress, for, like his chief Wright, he was an explorer—his mind was always moving on. The first of these, like several later contributions, was in the sphere of technical methods. Wright had introduced many ingenious procedures for micro-measurement by means of capillary glass tubes, rubber teats, and calibration by mercury. Fleming was quick to see that these could lend themselves to the new diagnostic methods for the detection of syphilis, which had been developed by Wassermann and others in Germany. His modification made it possible to carry out the test with half a millilitre of the patient's blood, derived from a finger prick, instead of five ml. obtained from a vein.

Very soon other technical problems arose in connexion with syphilis, for Wright became greatly interested in Ehrlich's discovery of the wonderful curative properties of dioxy-diamino-arsenobenzene dihydrochloride, known more familiarly as 'Salvarsan' or '606'. This had to be given by injection into veins—an almost unknown procedure in those days. Fleming undertook this work, and in one of the first reports to be published in English he described a satisfactory technique, and the results he had obtained in 46 patients.

Then came the First World War and Fleming at once became involved in quite different problems. It was quickly apparent that bacterial infection of the terrible wounds caused by high explosives was going to cost us very many lives and limbs. Wright was called upon to set up a laboratory in France for the study of these infections, and he took Fleming with him—with the rank of Captain, R.A.M.C. That first war-time laboratory for medical research was established at the Casino in Boulogne (since destroyed in the last war).

Fleming's first report upon his findings, early in 1915, stressed the great number of microbic species present in the wounds—some of them quite unfamiliar to most bacteriologists at that time; but it also showed that in the most serious infections, when the patient's blood stream was invaded, streptococci were chiefly responsible. It seemed probable that many of the wound infections were caused by microbes carried on fragments of clothing, mud, etc., which had been driven deeply into the tissues by the projectile.

These early observations of the wounds also brought out another important point, viz., that thorough application of the antiseptics in common use, either at the Front within a few hours of wounding, or somewhat later at the Base, was not sufficing to check bacterial infection, as many surgeons

had hoped it would. To Wright this was not surprising but he, and Fleming, had to spend many months of strenuous work investigating the matter in order to convince the surgeons that they were on the wrong road.

They concluded (Wright and Fleming) that two factors were chiefly responsible for the failure: First, the antiseptics did not reach the microbes because these had very often been implanted deeply in the substance of bones, cartilage, muscles, etc. And, secondly, the antibacterial potency of the solutions used was very much, and very quickly, reduced by combination with albuminous and cellular elements in exuded lymph, pus, blood, and the fixed tissues in the wounds; and the solutions thus reduced in strength were sometimes actually harmful, because they destroyed the patient's leucocytes, which, if unharmed and given favourable conditions, constituted a very effective natural defence mechanism.

The intellectual work which lay behind the formulation of these two important conclusions (and others) was almost entirely Wright's (he 'saw with his mind' where the truth must lie,) but Fleming, who shared in the work, made valuable contributions to it—especially in the technical sphere. It was he who made and experimented with 'artificial wounds' by drawing out spikes on the lower end of test tubes and filling them with serum, or serum broth, implanted with all the organisms from a dirty wound. When the latter had grown out freely the fluid was emptied out and the tube refilled with an antiseptic solution and left, sometimes as long as 24 hours. When this in turn was thrown out and replaced by sterile culture fluid a new outgrowth of microbes always occurred. Evidently the antiseptic had failed to reach and kill the organisms in the tips of the glass spikes. Most of the wounds caused by high explosives had far more inaccessible corners than Fleming's spiked tubes.

Another simple device which Fleming adapted (with due acknowledgment to its author, Dr Beattie) to the investigation of antiseptics was that of covering fluid cultures of a suitable gas-forming organism with melted Vaseline. As growth proceeded the confined gases pushed up the column of Vaseline and their volume gave a rough indication of the amount of growth. By this trick it was easy to demonstrate the greatly diminished potency of many antiseptics in albuminous fluids such as serum; and also the surprising fact that, with certain concentrations of many commonly used antiseptics (including carbolic acid, iodine, hypochlorous acid and sodium hypochlorite and also chloramine-T) bacterial growth was actually *increased*. (By this same device Fleming was able to demonstrate that the *Clostridia* associated with gas gangrene gave a much more abundant culture when grown in symbiosis with aerobic organisms from the wounds, such as staphylococci and streptococci.)

Yet another facet of 'the antiseptic problem' was revealed when Wright and Fleming transferred their attention to the anti-bacterial efficiency of the leucocytes in an infected wound. Briefly, they found that, given favourable conditions, the leucocytes of pus or blood were able to kill very large

numbers of staphylococci and streptococci; and that, under the influence of antiseptics, this beneficent activity was often interfered with. Here again Fleming's genius for simple devices brought striking results. His 'impression cultures', i.e., cultures made by applying a cover glass lightly to a wound surface and then immediately transferring it to a plate of nutrient agar, often demonstrated very prettily the anti-bacterial effect of the pus. And when such impression cultures were repeated before, and at intervals after, flushing a wound with an antiseptic it was often found that the bacterial growth was greater in the later cultures. Apparently the antiseptic had destroyed the leucocytes which had been holding the microbes in check.

Strong support was given to that conclusion by other experiments carried out by Fleming after the War, using the 'slide cell' technique which Wright had then introduced. By suitably adjusting the number of microbes implanted into the blood in these cells it was easy to show that, when the normal proportion of leucocytes was present, they exerted a very considerable bactericidal effect; and that, when additions of antiseptics had been made to the blood, that effect was greatly reduced, or abolished altogether.

Fleming's masterly summary of his researches on wound infections is to be found in his Hunterian Lecture to the Royal College of Surgeons in 1919 and in his communication to the Royal Society in 1924 entitled 'A comparison of the activities of antiseptics on bacteria and on leucocytes'.

His preoccupation, along with Wright, on the physiological defence mechanisms concerned in wound infections prepared his mind in some measure for the discovery, in 1922, of the remarkable microbe-dissolving ferment in nasal secretions which he named 'lysozyme'. In a sense this was a double discovery: that of the unfamiliar lytic agent, and, by an extraordinary chance, that of an equally unfamiliar microbe which happened to be exceptionally sensitive to its action.

Fleming's account of it to the Royal Society told how he was making daily cultures from a patient's nasal secretion (his own in fact) in the course of a 'common cold'. Until the fourth day his cultures grew little or nothing, but on that day there appeared 'a large number of small colonies which . . . proved to be large Gram-positive cocci arranged irregularly but with a tendency to diplococcal and tetrad formation'.* With Wright's help he subsequently christened this microbe, which was previously unknown to him, the *Micrococcus lysodeikticus* (i.e., capable of being dissolved).

It is not quite clear what led Fleming to suspect that there was something in the nasal mucus which would exert a powerful lytic action on this microbe. Probably there were some areas on his plate cultures where the growth of the *Micrococcus* was inhibited or prevented by particles of mucus. At any rate he did suspect it, and his suspicion was confirmed, when he prepared a thick suspension of the microbe from a fresh culture and added to it a drop of

* Dr V. D. Allison, who collaborated with Fleming in much of his lysozyme work, tells me that Fleming thought these micrococci did not actually come from his nose, but were chance contaminants blown on to the plate from the air.

diluted nasal mucus. To his astonishment the suspension became quite clear in a minute or two.

Subsequent experiments showed that a similar solvent action on this sensitive microbe could be demonstrated with human tears, and sputum, and saliva, with extracts of many tissues of the human body, as well as with egg white and other animal and plant tissues.

Oddly enough no other microbe was found, nor, I think, has since been found, which was as sensitive to the solvent action as *Micrococcus lysodeikticus*, although many others, including most of those associated with human diseases, were acted upon and destroyed to a lesser extent. A very significant finding, the full importance of which is probably still not known, was that the ferment lysozyme could be obtained from human leucocytes. The bactericidal effects which Wright and Fleming had demonstrated during the war with leucocytes derived from human blood and from wound exudates may well have been due in part to this ferment action.

The actual discovery of lysozyme was not perhaps a great intellectual feat but it should be remembered that hundreds of bacteriologists all over the world had been examining nasal secretions for years in the hope of finding the organism responsible for 'the common cold', and none of them had hit upon this remarkable ferment. Fleming also failed to find the cause of colds, but his questing mind and perceptive genius undoubtedly opened a new and important chapter in immunology by the discovery of lysozyme.

And a few years later (1928) he discovered another anti-bacterial agent, which he named 'penicillin', which was also elaborated by a living organism. The manner of that discovery was very similar to that of lysozyme. In the course of day-to-day observations of some staphylococcal colonies on an agar plate culture he noticed that some of them which were growing in the neighbourhood of a contaminating mould, were becoming translucent—apparently undergoing lysis.

This at once arrested his attention as something unusual. He grew the mould (afterwards identified as *Penicillium notatum*) on the surface of nutrient broth and found that the latter had become strongly antibacterial—its growth inhibiting power for *Staphylococcus* was nearly three times that of carbolic acid. Further investigation proved it to have other remarkable properties: it was very readily diffusible, and it was almost non-toxic for human leucocytes and for laboratory animals.

These were just the properties which Fleming's earlier experience had led him to expect would be useful in the treatment of infected wounds. Unfortunately, in addition to these desirable qualities, the crude penicillin of Fleming's filtrates had one great disadvantage—it was very unstable.

Fleming was not a chemist, and he saw no prospect of being able to extract and purify the active principle. He did not, therefore, seriously pursue the possible usefulness of penicillin as a therapeutic agent, but it is quite clear from his first paper on the subject that such an application was in his mind. He wrote:

'Penicillin, in regard to infections with sensitive microbes, appears to have some advantages over the well-known chemical antiseptics. A good sample will completely inhibit staphylococci, *Streptococcus pyogenes* and *Pneumococcus* in a dilution of 1 in 800. It is, therefore, a more powerful inhibitory agent than is carbolic acid, and it can be applied to an infected surface undiluted, as it is non-irritant and non-toxic. If applied, therefore, on a dressing, it will still be effective when diluted 800 times, which is more than can be said of the chemical antiseptics in use. Experiments in connexion with its value in the treatment of pyogenic infections are in progress.'

The last-mentioned experiments were not reported. It should be noted that at this time Fleming seems to have had in mind only the local application of penicillin—he did not foresee that (to quote Florey) 'It could circulate in the blood and body fluids in sufficient quantity to destroy sensitive bacteria in combination with the natural body defences without the least harm to those defences or to other tissues.'

Before turning to other problems Fleming did, however, show how even his crude filtrates containing penicillin could be usefully employed in bacteriological work—as a means of suppressing the growth of unwanted microbes in certain cultures, e.g., for the isolation of *B. pertussis* in whooping cough.

The subsequent history of the development of penicillin is too well known to need more than brief mention. After Raistrick and his colleagues had failed to extract and purify the active principle a fresh attempt was made by Chain and Florey at Oxford in 1940. By extraction with ether they succeeded in preparing enough partially purified material for preliminary trials of its antibacterial potency in laboratory animals infected respectively with virulent staphylococci, streptococci and *Clostridium septicum*. (Later experience showed that the penicillin used in these trials was only about 1 per cent pure.) These were remarkably successful and encouraged Florey and his team to seek further improvements in the extraction process. Solution in ether was replaced by an amyl acetate process, followed rapidly by acidification. By this and other means more stable samples of penicillin were gradually obtained, and undesirable impurities eliminated.

Fleming's findings as to the non-toxicity for laboratory animals and human leucocytes were confirmed and extended, and by 1941 favourable results were reported in the treatment of a few severe human infections. Others quickly followed, making it clear that penicillin was destined to take a unique position among the remedies effective against human diseases. Osteomyelitis and staphylococcal septicaemia, puerperal fever and other invasive streptococcal infections, pneumonia, infections of wounds and burns, gas gangrene, syphilis and gonorrhoea—all these and other human infections were treated with such happy and striking results as to justify large-scale manufacture. By 1944, thanks to the immense effort and energy of the American manufacturers and research teams, it was possible to treat every man wounded on the different war fronts, with incalculable benefit. When the war ended

supplies were adequate for the treatment of the civilian population in this country and North America. In the post-war years it was found that even bacterial endocarditis, which had previously been fatal in nearly 100 per cent of patients, could frequently be arrested by massive dosage.

All this, and much besides, has followed from the initial effort of Fleming on the one hand in 1928-29, and of Chain and Florey with their colleagues on the other in 1940-43. In view of this two-stage process of discovery, and the great importance of penicillin to the whole human race, it was perhaps inevitable that questions of priority and apportionment of credit should arise. It was pointed out,* with some truth, that Fleming's work with *Penicillium notatum* was only in line with that of several earlier workers on the Continent. One of these, Vaudremer of the Pasteur Institute in Paris,† had reported that prolonged contact with a mould, *Aspergillus fumigatus*, destroyed the infectivity of the tubercle bacillus; and, on the strength of that observation, he had treated more than 200 patients suffering from tuberculosis with an extract made from that mould. The results, as very briefly reported, were quite inconclusive. Similar claims had been made in respect of other moulds, and some bacteria. It is clear that antagonism between different micro-biological genera and species had been 'in the air' for some years, and Fleming himself admitted this in his Nobel lecture in 1945.

It is also clear that Fleming's work had brought to light a new and exceptionally potent example of bacterial antagonism, the active principle of which happened to be remarkably non-toxic for animal tissues, and for human leucocytes. There it was left for a decade, and had it not been for the discernment of Florey and the chemical know-how of Chain, and their combined patience and enthusiasm in overcoming many difficulties, penicillin might still be unborn as a practical therapeutic agent. The share of credit due to these two workers must therefore be a large one.

The award of the Nobel Prize for Medicine jointly to Fleming, Florey and Chain in 1945 happily recognized the credit due to all three; and in his latter years Fleming was the recipient of very many other honours, notably that of Knighthood, Fellowship of the Royal Society and of the Royal College of Physicians, in 1943, Commander of the Légion d'Honneur in 1945, the Rectorate of the University of Edinburgh in 1951, and honorary degrees from universities all over the world. His long and fruitful association with Sir Almroth Wright in the development of 'The Inoculation Department' at St Mary's Hospital was commemorated by its re-naming, in 1947, The Wright-Fleming Institute.

At the end of his life, from a heart attack on 11 March 1955, while still working in the Institute after his retirement from the Principalship, Alexander Fleming was buried among the illustrious dead in St Paul's Cathedral.

Fleming was a man of few words—and ideas did not greatly interest him.

* Florey, H. W. 1945, *Brit. Med. J.*, 2, 635.

† Vaudremer, A. 1913, *C.R. Soc. Biol., Paris*, 74, 278, 752.

Women, on meeting him for the first time, were often nonplussed by the unemotional, 'almost basilisk' stare, which some of them mistook for rudeness, while others were fascinated. But there was much friendliness and kindness behind that apparently cold exterior, as many can testify.

'Pain in the mind' was not the spur that drove him to do research, as it was with Wright, but rather an urge to do a job better than the next man. Competition was the breath of life to him. Wright and he made a fine team. Wright supplied the ideas, which Fleming usually received in silence, and then went away and devised some neat trick for working them out. It was a joy to see him at work at his bench—the slick, apparently casual, but always efficient technique. If anything went wrong he was quite unperturbed and proceeded to do the job better.

Wright, chaffing him, used to say that medical research was just a game to him—and there was some truth in that. On one occasion, when King George and Queen Mary were due to visit the laboratories at St Mary's, Wright wanted Fleming to display some of his bench technique. He did, but suspecting that it might not interest them very much, he also prepared one of his famous bacterial 'rock gardens' from all the available microbes which produced growths of vivid colouring. The story goes that when the Queen saw this she whispered to King George 'What is the use of this?' It was no use—but it amused Fleming.

Fleming was twice married, first, in 1917 to Sarah Marion, daughter of Mr John McElroy. She died in 1949. His second wife, whom he married in 1953, was Dr Amalia Coutsouris, who had worked with him at the Wright-Fleming Institute.

L. COLEBROOK

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W. M. H. Greaves

WILLIAM MICHAEL HERBERT GREAVES

1897-1955

WILLIAM MICHAEL HERBERT GREAVES was born in Barbados, B.W.I., on 10 September 1897, the only son of Dr E. C. Greaves, M.B., C.M.(Edinburgh). He was a pupil at Lodge School, Barbados, from 1905 to 1915, the last three years being spent in the VIth form doing mathematics and English only. Then after a further year, at Codrington College, he came to England with the Barbados English Scholarship in Mathematics, entering St John's College, Cambridge in 1916. He read mathematics and was a Wrangler with distinction in Part II of the Mathematical Tripos in 1919, being awarded the Tyson Medal in astronomy. Two years later he gained a Smith's Prize and was also elected to an Isaac Newton Studentship. In 1922 he became a Fellow of St John's College.

Greaves started astronomical research under the late H. F. Baker, F.R.S., then Lowndean Professor of Geometry and Astronomy at Cambridge, and during the first few years was concerned mainly with problems in classical dynamical astronomy. One of his characteristics was to undertake any task with an almost painful conscientiousness, without much regard for the labour involved, and this may in part have grown out of his association with Baker, whose own conscience was a hard taskmaster. Greaves's first published paper established the existence of two families of periodic orbits for an infinitesimal body moving in the orbital plane of two finite bodies in circular motion about each other. He next investigated two problems of commensurate, or nearly commensurate, motions in the solar system, the first that of long-period inequalities in the movements of asteroids whose mean motions are nearly half that of Mars, the second the question of the divisions within Saturn's rings. Many years earlier Kirkwood had suggested that these divisions are due to near commensurability between the periods of the corresponding ring particles and those of the larger of Saturn's moons. The theory had recently been developed by Goldsborough, but Greaves discovered a number of difficulties. He showed that the process of clearance of ring particles from such a division by this means would be slow. He also pointed out that if solutions were required to be valid for long periods of time it would be necessary to proceed to higher approximations than Goldsborough's, and that these seemed likely to be too complicated for existing mathematical techniques.

During his period at Cambridge Greaves met E. V. (now Sir Edward) Appleton, F.R.S., and through him became interested in a family of differ-

ential equations which had arisen in the then new theory of the triode oscillator. In a first paper he dealt with equations of the form

$$\frac{d^2v}{dt^2} + f(v) \frac{dv}{dt} + \omega^2 v = 0$$

where $f(v) = -\alpha + \beta v + \gamma v^2 + \delta v^3 + \dots$ and $f(v)/\omega$ is small, and in a second paper studied somewhat more generalized equations

$$\frac{dx}{dt} = \mu \xi \qquad \frac{dy}{dt} = \lambda(x) + \mu(\eta)$$

where $\lambda(x)$ is a function of x only, while ξ, η are functions of x and y , with a period 2π in y . μ is a constant. Greaves approached these equations as a mathematician, but was by no means indifferent to the physical side of the problem. It was his habit, if he had any doubts as to validity of the underlying physical assumptions, to say so very bluntly, and we can find him already taking this attitude in this early work.

At about this time Sir Joseph Larmor arranged that Isaac Newton Students should have the opportunity of working for a few months at the Royal Observatory, Greenwich. Apparently the only two to benefit by this excellent, but short-lived, plan were Greaves and the late L. J. Comrie, F.R.S. (subsequently Superintendent of the Nautical Almanac Office). Greaves's visit resulted in two papers in the *Monthly Notices of the Royal Astronomical Society*, dealing with some point of detail in the determination of stellar parallax by photography, the first discussing the effect of differential refraction in the earth's atmosphere, the second dealing with the second order terms in differential aberration. This introduction to the Royal Observatory paved the way to an appointment early in 1924 as second Chief Assistant, and it also started a fruitful collaboration with C. R. Davidson, F.R.S.

Before he left Cambridge Greaves published two more papers. The first was an attempt to apply the methods of celestial mechanics to the Bohr atom, assuming that the electron was subject to certain radial and transverse forces, as well as to the electrostatic attraction of the nucleus. This was merely one of many attempts being made at that time to meet difficulties with which the Bohr atom could not easily deal, which were quickly forgotten a year or two later when the newer quantum mechanics appeared. The other paper dealt again with the triode oscillator, being an early investigation into the stability of an oscillating radio circuit. Neither of these problems was investigated by Greaves again, although memories of 25 years earlier were probably a factor leading him to encourage the Royal Astronomical Society to take a helpful interest in radio astronomy when this first appeared in the 1940's.

The move to Greenwich was a turning point in Greaves's career. Until now he had been primarily a mathematician, a theoretician following the classical traditions of nineteenth century astronomy. At Greenwich he was brought into close touch with observational astronomy, and his interests

swung to those of a practical astrophysicist. This change, made also by other astronomers of about the same generation, was a natural result of the transformation in astronomy during the past half century, from a science confined mainly to the geometry and dynamics of planetary and stellar systems, to one concentrating on the physical structure and evolution of a much wider range of bodies, not only stars and planets but also galaxies, nebulae and interstellar matter. At no time was this transformation of astronomy proceeding faster than in the early 1920's; spectroscopy was developing rapidly in association with the first quantum theory and the Bohr atom, Saha had laid the foundation of the theory of ionization in stellar atmospheres, Milne was starting his work on radiative equilibrium, Eddington and Jeans were pressing on with the theory of stellar interiors, while the new large reflectors on the Pacific Coast were beginning to produce a fresh wealth of observational data.

Within a year of his arrival at Greenwich Greaves published with Davidson a preliminary note on the determination of effective stellar temperature, the start of many years of work on this difficult observational problem. At that time it was a matter of faith among practically all astronomers that a star emits what is essentially a black body spectrum, blemished, but not gravely disturbed, by absorption lines. Greaves himself admitted in later years that he started the Greenwich colour temperature work fairly firmly convinced of this idea, and only abandoned it after a long fruitless search for what he supposed must be undetected sources of systematic errors in his measures. Today the problem appears more complicated and it seems that a stellar atmosphere may have almost as many 'temperatures' as there are methods of measurement. Although even now there is still much which cannot be explained there is at least a general realization, for example, of the importance of the discontinuities at the heads of the Balmer and other hydrogen series, of the extent of the enormous wings of the hydrogen lines in *A* type spectra, and of the 'blanketing effect' of great numbers of intense metallic lines in the spectra of cooler stars.

In their first paper in 1925 Greaves & Davidson exposed most of the technical difficulties of the problem: that accurate allowance must be made for light lost in passing through the earth's atmosphere, that essentially what is measured is the reciprocal of the temperature, that available laboratory sources were all of comparatively low temperature, and that comparatively small errors of standard lamp temperature led to large errors of star temperature. For subsequent work the team was joined by E. Martin. Their method can be summarized briefly as follows: For any black body the intensity of radiation as a function of wavelength is given by

$$I_{\lambda} = 2\pi hc^2 \lambda^{-5} / (e^{hc/\lambda T} - 1)$$

or, as it is usually written for this work,

$$I_{\lambda} = c_1 \lambda^{-5} / (e^{c_2/\lambda T} - 1)$$

where $c_2 = 1.432$ cm deg. and c_1 is independent of wavelength. For any two black bodies

$$\ln \frac{I_\lambda''}{I_\lambda'} = \ln (e^{c_2/\lambda T'} - 1) - \ln (e^{c_2/\lambda T''} - 1)$$

$$\frac{d}{(d\lambda)} \ln \frac{I_\lambda''}{I_\lambda'} = \phi' - \phi'' = \Delta\phi \text{ say}$$

In the range of λ and T encountered in practice ϕ is very nearly a constant, With a mean wavelength of 0.5μ and with c_2 now 14320 micron deg.,

$\phi = \frac{c_2}{T}(1 - e^{-2c_2/T})$ is known as the spectrophotometric gradient.

Regions of stellar spectra are measured which are as free as possible of absorption lines. The greatest technical difficulties arise in reducing the relative gradients to temperatures, which involves determining the gradient of a standard lamp with the same apparatus, and in finding the transmission of the earth's atmosphere for each separate star observation, in every measured wavelength. At the time the work started it was not realized how variable the terrestrial atmosphere is for precise photometry; today few would be rash enough to attempt such measurements in the immediate neighbourhood of a large city such as London.

The results of many years of measurement, by gradually improving methods, were summed up in two volumes of *Greenwich colour temperature observations*, appearing respectively in 1932 and 1952. The style of the first volume suggests that Greaves wrote much of it himself; the second, much delayed by the war, appeared long after he had left Greenwich and although owing much to him, is not the direct product of his hand. The zero of the gradient system was referred to a group of 40 stars not appreciably affected by interstellar absorption and for these the standard value in the visible spectrum is still that of the Greenwich work, $\phi_0 = 1.11 \pm 0.08$, corresponding to a colour temperature of about 15000° K.

During the course of this work an investigation was made into the yellow *B* stars. It is now generally accepted that the chief cause of reddening of these hot and intrinsically blue stars is interstellar dust particles, but when the first Greenwich measures were made this was by no means certain. Greaves quite early suspected that interstellar matter was responsible for the reddening, some years before Trumpler published compelling evidence demolishing the then conventional astronomical belief that space is practically perfectly transparent. Nevertheless Greaves was already convinced that there could also be an important reddening intrinsic to the star itself, especially with *B* stars having emission lines, and could point to a large change in the colour of γ Cassiopeiae when it brightened in 1936. The reddening was shown to be correlated with the strength of the hydrogen emission lines and evidence was

quoted showing that *Be* stars generally have a tendency to be redder than ordinary *B* stars.

The Greenwich spectrophotometric work during this period also included measures of the spectrum of Nova Herculis, the first spectrogram of which was obtained by Greaves's colleague, E. Martin, on the morning of discovery. The chief measures, which covered the first few months of development of this unusual nova, were of the lines $H\alpha$ and $H\beta$, and include one of the very few serious attempts to measure the brightness of a nova in absolute units, in the light of one important spectrum line.

Spectrophotometry of the continuous spectrum was not by any means Greaves's only interest at Greenwich. In 1925 he analyzed proper motions and radial velocities of stars to try to clear up the discrepancy between positions of the apex of the solar motion determined respectively by Boss from proper motions and by Campbell from radial velocities. He confined his attention to stars for which both types of motion had been measured, but despite this and some other refinements of method the results continued to show greater differences in the declination of the solar apex than internal consistency would suggest. No complete explanation could be found, and it is fair to add that obscurities of this kind are still awaiting clarification today.

Another line of work, in what has proved since a more fashionable field, was concerned with the connexion between magnetic storms and sunspots. The systematic records started at Greenwich in 1874 had already proved their value in Maunder's investigation of this problem. With H. W. Newton, Greaves extended Maunder's work to include the data of another quarter of a century. They showed that over half of the storms with a range greater than 300γ in either horizontal or vertical component of the magnetic field began within 4 days of central meridian passage of a large spot group, while 16 of 17 very great storms were associated with large spot groups. They also found that despite a clear general connexion with sunspots there could be individual storms in the absence of visible spots. They showed too that although magnetic storms on the whole have a tendency to recur after one solar rotation, this is not true of large storms. Although this was his only published work in this field, Greaves took an active and helpful interest in the Greenwich magnetic department throughout the whole of his time there. His continued interest in magnetic problems was shown as late as 1949, in a Presidential address given when presenting the Gold Medal of the Royal Astronomical Society to Professor Sydney Chapman, F.R.S.

Early during his Greenwich period Greaves became a great enthusiast for flying, a form of travel which he advocated tirelessly to the end of his life. With F. J. Hargreaves and W. M. Witchell he observed the total solar eclipse of 1927 from a plane at about 11000 feet, the machine being chartered for this purpose by the *Daily Mail*, from Imperial Airways. No very significant scientific result came from this pioneer flight, but it drew attention to the possibility of obtaining a clear sky even when the weather prospects on the ground are very poor. There are of course severe technical difficulties which

are not encountered in conventional eclipse observing; in fact it was many years before others took up the challenge and observed another eclipse from the air, and only in very recent years, for problems of the outer corona, has full advantage been taken of the excellent sky conditions available at high altitudes.

In 1938 Greaves was appointed to succeed R. A. Sampson, F.R.S., as Astronomer Royal for Scotland, a dual post, the occupant being Professor of Astronomy in Edinburgh University, as well as in charge of the Royal Observatory, Edinburgh. Before he had properly settled down in his new surroundings the war came and ordinary astronomical research was laid on one side. An independent time service was set up at Edinburgh, in case the regular Greenwich time service should be completely disrupted, and was run by Greaves with the help of only a very small staff throughout the whole war. This led to an interest in the characteristics of free pendulum and quartz crystal clocks. With L. S. T. Symms he analyzed the errors of a number of such clocks, classifying these errors according to whether they arose from accidental changes of phase, or of rate, or of some combination of these. The pendulum clocks possessed all three types of error, but for the quartz clocks the second and third errors were too small to be detected. The general result was that quartz clocks are superior to the best pendulum clocks, a view which has since been generally accepted.

During the 1940's, even after the war had ended, circumstances did not allow Greaves the opportunity of following much research. He published several lectures or review articles during this period and one or two quotations are worth making here. In 1940, of the spectrophotometry of the continuous spectrum: 'We are in fact discussing a very complicated and correspondingly interesting problem, and if the work of the last fifteen years has exposed the fallacies of the very simplified scheme of things which many astrophysicists used to take for granted, it has at any rate put us on to what is believed to be the right path, and has assured us of continued interest for many years to come'. In 1945, in the Halley Lecture, discussing the physical structure of stars: 'We are only in the first stages of a long process of inductive enquiry in which we try to invent plausible hypotheses, deduce their consequences by mathematical reasoning and confront these deductions with the test of observation'. And a little later in the same lecture: 'In the present state of astrophysics the observer knows quite well that when he is presented with new theoretical predictions there is a heavy probability that they will be found to be incorrect.' In 1948, on reddened hot stars: 'although the existence of intrinsic reddening in addition to space reddening has not been established beyond all doubt, the balance of evidence favours the idea that reddening by interstellar scattering is not the only process involved. . . . We may well be forced, as a result of a developed theory of scattering, to ascribe some of the reddening to some effect intrinsic to the stars. . . . But one thing does seem to be very clear, and that is that we do not yet know enough of the observable facts.'

One of Greaves's last papers is a report of photometric measures of the lines

in stellar spectra of type *B1* and is noteworthy for two reasons. In the first place the unconventional method was followed of taking measures at fixed wavelengths and combining the results of independent photographs without preliminary smoothing. There is no doubt both that this method is theoretically sound and that it can give greater accuracy than is obtained from the conventional method, where there is a preliminary smoothing of a microphotometer record. Faint lines are especially well revealed in this way; indeed one of his colleagues in this work, R. Wilson, was able by this technique to show for the first time the existence of the $5^2G-n^2H^\circ$ series of HeII in *Oe5* stars, although the line depths are only about one per cent of the continuum. The second point is that although more accurate this procedure is tremendously laborious. Characteristically enough Greaves took little notice of this fact; nor did he follow the fashionable trend and discuss the possibility of lightening the burden by suitable machinery.

Astronomy at Edinburgh, as everywhere else in Britain, suffered a serious setback from the almost complete cessation of normal activities during the war, and the lost momentum was only slowly regained. Greaves had, however, started a programme of re-equipment of the Royal Observatory and had already obtained a combined solar spectrograph and spectrohelioscope, a microphotometer of unusual design, and a 16-24 inch Schmidt telescope intended for stellar photometry. Plans were well advanced for a large new spectrograph for the 36-inch telescope, but he did not live to see its completion. He died very suddenly on 24 December 1955.

Greaves' scientific work was marked by great conscientiousness and was dominated by the desire to make astrophysics an accurate, quantitative, science. He wasted no time over descriptive astronomy and had no patience with speculative theory. He never evaded hard work or unattractive duties; on the other hand, he avoided publicity and seemed indifferent to personal distinction or reward.

In person he was a large man, inclined to nervous and slightly clumsy movement. Although extremely serious minded when it came to matters of principle, he had a keen sense of fun, even frivolity, and one of my last views of him was on an Irish train where he was sharing a boisterous children's game with his son. There is, too, an engaging story of Greaves in his first snowstorm, not long after he had entered Cambridge, dancing about St John's Second Court with a fellow West Indian, trying to catch snowflakes. He had an unusually good verbal memory and could tell a good story, although occasionally he tended to hammer the point home too much. His laugh was entirely his own; one might attempt to describe it as a quiet hissing cackle, something like air escaping intermittently from a water tap, accompanied by a low chuckle. After telling a story or making a point he had a habit of bending over and peering closely into the auditor's face, which the uninitiated found slightly disconcerting. Another personal characteristic was his pipe-lighting ritual, the untidy filling of the bowl, the pocket patting

to find matches, the verification of their existence by shaking the box near one ear, and the heavy flourish with which the match finally was struck.

Loyal, generous, disinterested, he was a practical democrat who thought as much of the welfare of his juniors as of the opinions of his seniors, and obtained the respect and affection of both. He was unusually free of self-consciousness and somehow managed to convey a suggestion of fearlessness, both physical and moral. In his personal manner he often defied the conventions, but his idiosyncrasies were for the most part only skin-deep, while although he seemed unaware of it they were a never failing source of amusement to his friends.

He became a Fellow of the Royal Society in 1943.

He was a strong supporter of the Royal Astronomical Society from the time that he joined it, in 1921. He served as a Council member, as a Secretary for seven years, as Vice-President and in 1947-49 as President. He was an Editor of *The Observatory* for eight years. For several years he was a Secretary and then Recorder of Section A of the British Association. In the International Astronomical Union he was President of Commission 25 (Stellar Photometry) and had earlier presided over a Sub-Commission on Standard Star Magnitudes. He had also done much thankless work for the I.A.U. as Chairman of the Nominating Committee.

At Edinburgh he served as Secretary and as Vice-President of the Royal Society of Edinburgh. For many years he was chairman of the committee administering the Robert Cormack Bequest for the encouragement of astronomy in Scotland.

With a strong vein of commonsense Greaves was a good committee man and always undertook such duties very seriously. He could make up his mind and stick to it, expressing his views forcibly and clearly, sometimes a little heavily, but never in such a way as to give offence. His colleagues respected his honesty and sincerity even when they could not always agree with his opinions.

He married in 1926 Caroline Grace, daughter of H. D. Kitto of Whiteshill, Gloucestershire, who survives him with an only son.

The writer wishes to express his thanks to Mrs Greaves, Dr J. Jackson, F.R.S. and Mr H. W. Newton, for their help in preparing this notice.

R. O. REDMAN

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A. L. Hall

ARTHUR LEWIS HALL

1872-1955

ARTHUR LEWIS HALL was born at Birmingham on 10 January 1872, the son of William Hall and his wife Mary Ann, born Smith. He died, in his eighty-fourth year, on 13 August 1955, at his home in Pretoria, South Africa.

His early years were spent in Germany where he received his early education, at Bonn, Freiburg, and Schwehn in Westphalia. At the age of 19 he won a scholarship at University College, Bristol, for further study. He then went to Gonville and Caius College, Cambridge, where he won a College Scholarship and the University Harkness Scholarship for Geology.

Graduating from Cambridge with first-class in both parts of the Natural Sciences Tripos he joined, in 1899, the staff of Dulwich College on the science side and was head of that side when he resigned to take up a Colonial Office appointment as geologist to the Geological Survey of the Transvaal, which had been re-constituted under H. Kynaston after the termination of the Anglo-Boer War of 1899-1902. In August 1900 he had married Rosalie, third daughter of Mr and Mrs F. G. Powell of Clifton, Bristol.

Hall arrived in Pretoria on 16 January 1903, to begin a period of 29 years of service as a geologist in the Government service—first as Geologist to the Government of the Transvaal and, from 1910 to the date of his retirement in 1932, as Geologist and, later, Assistant Director of the Geological Survey of the Union of South Africa. At the time of his arrival, gold and coal were almost the only minerals of note that were being produced in the Transvaal and few details of the geological features of the country were known. During the next fifty years, the advances both in knowledge and in mineral output were remarkable; and in not a few of these advances Hall's geological work was largely, if indirectly, one of the stimulating forces.

Beginning his study of Transvaal geology in the immediate environs of Pretoria, which lies close to the southern boundary of the Bushveld igneous complex and which stands on the northerly-dipping quartzites and shales that form in this area the floor of the complex, Hall worked for some years in an ever-widening area to the east, north and west of the capital, mapping and reporting on the outcrops examined by him during each field season. In those early years, field geologists had but poor base maps on which to plot their results and transport was limited to animal-drawn conveyances. The country was more sparsely populated than at present; roads were poor; rivers had to be crossed at fords; the health hazards arising from mosquitoes

and ticks were far greater than they are today, and draught-animals had to be carefully guarded against epidemic diseases. Repairs to vehicles damaged in the field had to be made by the geologist; Hall, a man of powerful physique, used to relate how, on one occasion, he had to fetch and carry a new wagon axle for miles to his camp in order to re-fit the vehicle for service. Outcrops and boundaries had to be traced on foot; and, presumably in response to the demands of statistically-minded office-wallahs, each *Annual Report of the Transvaal Geological Survey* solemnly lists the total area mapped by a geologist, the number of miles of rock-boundaries plotted by him, the distance covered by his transport animals, and the mileage that he had foot-slogged. No figures seem to have been required in respect of the total changes of altitude through which the geologist had to transport himself. One can easily visualize Hall, ever with an eye for details and a sense of accuracy, handing in to the proper authority his annual totals with an offer to substantiate their authenticity by reference to his daily notes and with a twinkle in his eye. The author well remembers being advised by him 'If there are rules and regulations stick to them, however silly they may be. By doing so, you save time and trouble and you do not make the clerical fellows think—a thing they are not paid to do.'

In addition to the normal geological mapping, Hall's attention was given to the study of the mineralization exhibited by the rocks that he encountered and to the possibilities of economic exploitation of mineral deposits associated with the great Bushveld complex. During his first eight years in the Transvaal he reported on the geology of the Bushveld tin-fields, on magnesite deposits at Malelane in the Lowveld, and on gold occurrences at various places outside the Witwatersrand, including those of the Pilgrim's Rest fields. The description of the Pilgrim's Rest gold mining area (published in 1910) was the first of the many memoirs by Hall that enriched South African geological literature. The second of these was that on the Murchison Range and District, in which he showed that the auriferous and antimonial deposits of the range were intruded by the Older Granite and considered that the granite-syenite-pyroxenite complex of Palabora (now spelled Phalaborwa), with the Lulukop limestone, constituted a younger petrographic province 'not unlike the Bushveld Plutonic Complex'. The Lulukop limestone was thought to be a mass of metamorphosed sedimentary limestone caught up in the complex—a view accepted by Shand, du Toit and others; it is only within the past few years that intensive study has shown it to be a carbonatite of magmatic origin.

The next major publication was the memoir on the geology of the Barberton Gold Mining District (1918), based on seventeen months of field work in a mountainous belt of country ill-served by roads, together with the petrographical study of a large amount of material. For the sediments and volcanic rocks older than the Older Granite of this area, Hall used the name 'Swaziland System' based on A. Schenck's term 'Swasischichten', and subdivided the system into a basal Onverwacht series of lavas and tuffs, a

middle Moodies series of slates, greywackes, conglomerates, banded iron-stones, and quartzites, and an upper group of basic schists with serpentines and other basic rocks which he called the Jamestown series. The structure is a complicated one, the sediments showing steep dips and considerable faulting with repeated folding. Subsequent detailed study of the area, aided by aerial photographs and large-scale contoured topographic maps together with greatly increased accessibility, has caused some changes in Hall's conceptions of the stratigraphical succession with refinements of nomenclature; but this memoir remains as an outstanding example of single-handed and single-hearted study of what had previously been almost *terra incognita* to the geologist.

The impetus given by the demands of the first world war (1914-18) to the development in the Allied countries of natural resources led the Government of the Union to undertake an assessment of South African resources, including those of base mineral deposits. To this work, the small staff of the Geological Survey turned its full attention; between 1918 and 1922 Hall produced memoirs on asbestos, on mica, and on corundum in addition to a number of subsidiary papers on these and allied subjects and the first of his invaluable bibliographies of South African geology—an alphabetically-arranged authors' index of papers published to the end of 1920. The companion subject index to this volume was issued in 1924.

When, after world war I, a large gallery in the new Transvaal Museum building was made available to the Geological Survey as an exhibition hall, the task of organizing a display of geological and educational value was undertaken by Hall. Into this work, which included the designing of combined exhibition and storage cabinets as well as the assemblage of material for display, Hall threw himself with his customary enthusiasm and, with the limited financial means at his disposal, succeeded with but little help in making available to the public an attractive and representative collection illustrative of geological science and of the geological history of South Africa. In spite of subsequent refinements and alterations, the geological museum in Pretoria remains substantially Hall's work.

In 1922 the Shaler Memorial Expedition, consisting of R. A. Daly, C. Palache, G. A. F. Molengraaff and F. E. Wright, arrived in South Africa with the principal object of studying the Bushveld igneous complex. Hall was the natural guide to this visiting group for, as he himself stated, ever since 1903 hardly a field season had passed without one or other aspect of the complex occupying his attention. For five weeks he conducted the expedition through Sekukuniland in the north-eastern part of the Transvaal 'where nature has traced the framework of the complex and exposed its fundamental problems on a scale not approached by any other section of the Bushveld'.

Stimulated by the suggestions and encouragement afforded him by Daly and his colleagues, by fresh facts furnished during the intensive period of prospecting and mining operations that followed the discovery of platinum in the complex towards the end of 1924, and by the discussions held in the

field during his leadership of an important excursion (C19) at the time of the Fifteenth International Geological Congress in 1929, Hall set himself, with considerable misgiving, the task of compiling a memoir on the complex. This memoir was published by the Geological Survey in 1932, after Hall had retired from that body on reaching the age limit.

The memoir is a volume of more than five hundred pages. The first quarter of the work is devoted to summarizing the relevant information that had been published up to 1932, Hall's reason being 'It is evident that the present subject is too vast to fall in all its phases within the experience of a single observer.' The remainder is divided into 14 chapters, at the end of each of which is given a summary of the author's views on the facts presented. Chapter xiv is devoted to the geological history of the complex and envisages the possibility of alternative views.

No one realized better than Hall that his work was not the final word on the subject. The closing paragraph of his introduction reads: 'The Bushveld Igneous Complex is a vast treasure ground which for many years to come will provide the mineralogist, the petrographer and the geologist with a rich field to dig over'. (He might have added the mining man.) 'In submitting to the indulgence of the reader this attempt as a first approximation to the truth, the writer would recall the words of a great thinker: "Nothing would be done at all if a man waited till he could do it so well that no one could find fault with it. What, then, can I desire or pray for but this, that what I may say will be blessed to those who hear it, and that what I might have said better may be blessed to me by increasing my own dissatisfaction with myself."' From 1932 onwards, many others have set themselves to probe one or other of the secrets of the complex; facts have been noted or re-noted, and ideas differing from those adumbrated by Hall have been evoked to explain these facts; but the whole truth is not yet clear and Hall's 'first approximation' will remain as a monumental standard work which no geologist can dare to ignore.

It was during Molengraaff's visit in 1922 that he and Hall studied together the geology of the Vredefort dome, the details of which were being mapped by L. T. Nel. Here the fundamental problems were the relationships between the supposed Older Granite exposed in the middle of the 'dome' and the surrounding broad girdle of Witwatersrand sediments which were steeply dipping and even overturned and were at the same time much more highly metamorphosed than in the type area of the Witwatersrand near Johannesburg. Two important papers (1924 and 1925) were written on this problem. The authors concluded that the Older Granite showed no intrusive relations to the sediments; that there was present a Younger Granite, of somewhat alkaline type; and that the metamorphism displayed by the sediments was in part a load metamorphism and in part thermal change dependent on the intrusion of the later magma. The tectonic features were attributed primarily to updoming of the central Older Granite which was in part assisted by the active rising of the younger magma.

Hall attended the xivth International Geological Congress in Spain in 1926, where he announced an official invitation from the South African Government to the Congress to meet in the Union in 1929, the same year in which the British Association had decided to meet in South Africa. The invitation was accepted, and Hall on his return home was nominated as the General Organizing Secretary for the xvth Congress. His work in this capacity was outstandingly successful; no detail of organization was too small for his attention—and only those who have taken part in arranging a large international scientific gathering can appreciate the number and variety of the details that have to be considered and settled if smoothness of operation is the objective. During this period of intensive preparation, Hall found time to compile the guide-books for three important post-Congress excursions, including those to the Vredefort area and to the eastern part of the Bushveld complex. In presenting to him the Murchison Medal in 1930, the President of the Geological Society of London specifically referred to his leadership of the Congress excursion to the Bushveld complex 'with which his name will always be associated'. Hall's Fellowship of the Geological Society dated from 1900.

Between the end of the Congress in 1929 and his retiral from the Geological Survey on the attainment of his 60th birthday in 1932, Hall prepared for publication the *Proceedings of the xvth Congress* and the *Symposium on the Gold Resources of the World*. He revised and published his earlier memoir on asbestos and completed for publication the *Author's index of the geological bibliography for the years 1926-1930 (inclusive)*. This bibliographical series was completed to the end of 1935 by a further *Author's index* (1937) and an additional *Subject index* (1939).

Throughout his period of official service Hall had maintained, largely for his own information, an index of chemical analyses of rocks, minerals, ores and other geological materials emanating from southern Africa *sensu lato*. After his retirement, he offered to bring this into a form fit for publication under Government auspices; in 1938 publication as a memoir was effected, in a volume of 868 pages containing nearly 5700 annotated analyses and supplied with an index. This was the last of Hall's major contributions to geology to appear; and his work as a productive geologist may be said then to have ended. He attended the xvth International Geological Congress in the United States in 1932 and the xviith Congress in the U.S.S.R. in 1937, participating in long excursions in each country; but failing sight began to worry him and he decided to abandon active geological study.

From 1925 onwards, the value of his work had been recognized by several highly-prized honours. Cambridge University awarded him the degree of Sc.D. (1925). The Geological Society of London conferred upon him the Murchison Medal. He was elected Fellow of the Royal Society (1935). The Geological Society of America made him a Corresponding Member in 1934. The Geological Society of South Africa, of which he had been President in 1913-14, selected him to be the first recipient of its Draper Memorial

Medal, awarded a few weeks after his retirement from the Public Service in 1932.

A. L. Hall was, however, more than an outstanding geologist. He was a very human person, intensely interested in the welfare of his talented family, of his official colleagues and their families, and of those with whom he came into official or private contact. To the rank and file of the public servants of his time, he was known as one whose guidance did much to shape the early policy of the Public Servants' Association and who was largely responsible for persuading the professional and technical officers of the Service to join the Association in preference to forming themselves into a separate body. For some years he was a regular attendant at the Association's annual conferences, where his sane and lucid remarks received the greatest attention. He held office in the Association as Vice-President in 1922 and again in 1934, and was one of the original members of the Public Service Advisory Council.

Possibly, however, outside his scientific work he is best remembered by a considerable number of people for his intense interest in all that is best in music, an interest that he maintained unabated to the end of his life. His instrument was the violin, which he began to play in his early years in Germany. He particularly loved participation in the intimate rendering of chamber music—trios, quartets, or quintets—and in the performance of orchestral works with a group of musical friends. For many years no orchestral concert was given in Pretoria, if Hall was in the city, without him taking his place among the first violins; and many were the times when the orchestral parts of works to be performed were supplied from his comprehensive library. He inspired music festivals commemorating the Masters he so greatly admired. On the occasion of the centenary of the birth of his favourite composer, Brahms, he insisted on what he called the privilege of designing and paying for what must surely be the most artistic programme-textbook ever presented in Pretoria. Many now well-known artists were given their first opportunity of performing for the elect in the city's musical world through his good offices; and he gave every possible encouragement to younger people who seemed desirous of improving their musical knowledge and talents.

A forthright character, with a large physique and a somewhat gruff manner, Hall wasted no time with the helpless amateur or the pretentious professional, either in geology or music. He studied, however, the personal predilections of his friends and delighted in assisting to satisfy their individual fancies by occasional appropriate and unheralded gifts of some food delicacy, the score of a work by Bach, or an out-of-print geological work from his own library. To his friends, kindness and gentleness were the most prominent facets of a many-sided character.

The last few years of his life were saddened by increasing blindness and, even more, by the deaths, in the prime of life, of two of his brilliant sons. He is survived by his widow, a married daughter, a married son, and several grand-children, as well as by a few of those who were his colleagues and

whom he had made his friends. South African geology and Transvaal music have both been greatly enriched by him.

The compilation of this tribute has been made the easier by the help given by Dr Hall's daughter, Mrs Brunton Pollock, and by his old musical friend Professor Gerrit Bon, F.R.C.O.

S. H. HAUGHTON

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George Stickling

HENRY GEORGE ALBERT HICKLING

1883-1954

HENRY GEORGE ALBERT HICKLING, who always called himself 'George Hickling', was born in Nottingham on 5 April 1883. His school education in Nottingham ended at the age of sixteen, and he then lived for three years in Arbroath with a sister and brother-in-law. Here he developed an interest in geology, by observation of the structures visible on the sea shore and in the cliffs of Angus, and by walks in the Grampians. In 1902 he became a student in the University of Manchester, and worked under Professor Wm Boyd Dawkins and Bernard Hobson, gaining 1st class honours in geology in 1905. A year later he became a demonstrator in the small department.

The Geology Department was housed, with those of Botany and Zoology, in a building attached to the Manchester Museum, and the close association between the staff and students of the three departments naturally led to very wide general interests in those fortunate to work in them. At that time palaeobotany had become of real interest to 'structural botany', and the discovery by Scott and Oliver in 1904 that *Lagenostoma* was the seed of a plant with fern-like leaves and a peculiar anatomy roused wide interest, especially in Manchester where there were large collections, and where Williamson had worked. Thus it is natural that Hickling published admirable accounts (1907 and 1910) of the detailed structure of two Calamitean cones; to these nothing has since been added. In 1906 he saw in the Nottingham Museum tracks of a reptile from a sandstone interbedded in the Magnesian Limestone of Mansfield, and described one form immediately. Somewhat later (1909) he discovered several more types of track from the same place, and realizing that they were the only footprints known of certain Upper Permian age, examined all other accessible footprints also regarded as 'Permian'—from Penrith, Dumfries and 'Elgin'. Of these he gave a summary account, with excellent outline figures of individual prints and tracks, showing that all those from these places, whilst largely similar to one another, differed entirely from all known Triassic tracks and from those of Carboniferous age. Nothing has since been added to our knowledge of Upper Permian tracks.

In 1908 and 1912 he published short accounts of the Old Red Sandstone of Forfar, which still remain as our main source of knowledge of these extraordinary sediments, and of the distribution of fossils in them. Later his student D. A. Robson described the lavas which they contain, and published a map partly based on Hickling's observations. Hickling made considerable collections of plants from the Old Red Sandstone of Angus. A Middle Old

Red Sandstone plant from Caithness, preserved complete in a slab, was later called *Hicklingia* by Kidston and Lang in recognition of his work on such things. In 1917, with A. W. R. Don, he explained the structure of the remarkable Old Red Sandstone plant '*Parka decipiens*'.

In 1910 Thomas Henry Holland took charge of the Department of Geology in Manchester, and his great interest in economic matters spread to Hickling, who began to study in detail the geology of Lancashire. But as a preliminary Hickling, from the Geological Survey maps and the accompanying *Memoirs*, and other published sources, drew a new map of south Lancashire, designed to bring out the structure of the coalfield, and to illustrate how a more accurate one would enable much more dependable judgments to be made as to possible mineable coal reserves about the area. This map brings out the extraordinary scale of the faulting, and the high dips often found in the field. The paper, published in 1916, was intended not only to summarize what was known, but to make a case for a new survey necessarily based on mining evidence. It was successful and from then on Hickling was engaged in reducing colliery records to a form in which they could be used to make a map.

Holland soon became absorbed in the approaching war, and very much of his time was devoted to the affairs of the Officer's Training Corps; when war broke out he returned to India, and Hickling controlled the department until 1920. But Holland became President of the Manchester Geological and Mining Society, urging upon that body (which included some coal owners and all mine managers in Lancashire) the need for a new study of the geology of the coalfield. Mr John Gerard, the Inspector of Mines, gave most active and successful support to the view; his influence ultimately led the majority of coal owners and managers in the county to give Hickling the records of shaft sinkings and borings, and often actual mine plans.

Hickling recognized at once that the correlation of the individual coal seams in the eastern area of Oldham to Dukinfield was reasonably well understood, and so was that of the western area from Pendleton to Wigan, and that further west to Prestcote something was known. But the Manchester Coal Field worked seams unrecognized out of that area, which was isolated by enormous faults of a thousand yards throw. The only way of making correlations was to draw out each sinking section on a true scale with conventional distinctions of shale, sandstone, and so on. Direct comparison of such sections from one pit to its neighbour, if the sections are of considerable length, usually enables one to correlate each seam in two neighbouring pits. Usually miners recognize coal seams at once by peculiar qualities of a kind impossible to set down on paper; and such traditional identifications are very commonly correct. But the great faults of Lancashire were not readily crossed by such determinations. The key to the whole was the correlation of the seams actually worked under the middle of the City of Manchester, in the Bradford Colliery. Here the seam originally worked, the 'Bradford Four Feet', was traditionally identified as the 'Worsley Four Foot'. But another

seam, the 'Parker Mine', lying some thirteen hundred feet below the 'Bradford Four Feet', is equally like the 'Worsley Four Foot', and bears much more similar relations to the coals below it. Hickling concluded, rightly, that the 'Worsley Four Foot' is the 'Parker Mine', and thus carried his correlation across the whole of south Lancashire.

Hickling then considered how to represent the detailed information given by the sinking sections and mine plans so as to be most useful to the coal mining industry. South Lancashire is almost continuously covered by drift, largely Boulder Clay, from a few inches to two hundred feet thick, so that the surface on which this lies differs greatly from that actually exposed. Thus a traditional geological map, representing the outcrop of coal seams on the 'solid' surface, cannot be drawn in the absence of information about the drift, and is in any case of little immediate use. Hickling (following an example by Professor Fearnside in Yorkshire) therefore decided to draw a contour map of a single seam. This was based on shaft sections, mine plans (which are in effect actually contour maps) being used whenever possible. When the coal used (the Trencher-bone) was absent, its former position was estimated from that of some lower coal. The whole map, published in 1924, gives a picture of the structure of a very heavily faulted country, in which faults often hade at 45 degrees, so that they may remove coals from large areas. These losses, vital to coal miners, are vividly shown. The preparation of this map was done by Hickling, without assistance of any kind, at a time when he—single handed—was responsible for the conduct of the department throughout the war, and himself gave all the teaching in it to students of geology and to engineers. Thus Hickling unfortunately never discussed the significance of his map in terms of coal which could be won in new places. The Geological Survey later in all essentials confirmed his correlations of coal seams, and pointed out areas worth exploration. That the work was well done is an admirable example of his conscientiousness, and his great power of continuous hard work.

In 1920 Hickling moved to Newcastle-on-Tyne, and was kept very busy reorganizing a small Department of Geology, much understaffed. Thus he made few published contributions to the details of the local geology, though on several occasions he gave us admirable summaries of it. But in the words of his student and successor, T. S. Westoll, he added much to our knowledge of the country 'chiefly in the form of confidential reports on colliery and water supply problems, and among his effects are masses of sections and notes of very great value. The Hutton seam map is the only contour map he published, but in fact he had several others in hand, and we have a number of preliminary outlines, for instance, of the Great Limestone. His feeling for the geological structure was uncanny, and his memory excellent. It was commonplace to be told, when one was travelling in his car over Northumberland and Durham and had asked some question of the lie of the geological land, just how far beneath one's feet was the Hutton, or Brockwell seam, or Great Limestone; and to be told also that here one crossed a fault with locally

such-and-such a throw; and to discover at leisure that the information was sound.'

After he retired he produced in 1950 a most interesting study of the prospects of undersea coalfield extension off the Durham coast. This is based on a map, on a scale of one inch to a mile, of the Hutton seam represented by contours on its surface at hundred foot intervals. The contrast between this map, with its simplicity, and the absence of faults of more than five hundred foot throw, and that of the Lancashire Coalfield is most striking. It is in fact easy and probably justifiable to continue the map for a few miles into the unknown land below the sea. Whether coal so predicted exists depends amongst other matters on whether or not it had been cut out by erosion before the Permian rocks were deposited, and thus on the possibility of predicting the Permian base. Hickling held that what local evidence there was, with that in north Yorkshire, justified an inference, which he represented on a contour map. From this and the original map of the Hutton seam it is possible to estimate how much of the Coal Measures have been removed below the Permian. How far Hickling's conclusion (that considerable workable areas exist off the Durham coast) is justified, is now being examined directly by under-sea borings. Meanwhile in 1949 he wrote for the *Scientific Survey of North Eastern England*, published for a British Association Meeting, a most lucid and admirable account of the geology of north eastern England. But much of Hickling's life was devoted to a study of coal as a material.

In 1832, at a British Association Meeting at Oxford, Henry Witham exhibited sections which first showed that the cellular structure of plants survived in coals. In 1833 his friend, William Hutton, described the structures visible in a long series of coals in a paper to the Geological Society. This was published only in abstract, probably because its illustrations (water-colour paintings) were impossible to reproduce. The manuscript, with its drawings, and the sections on which it was based, survived and in 1935—more than a hundred years after it had been read—was published by Hickling, with photographs made from the original sections. This work had been completely lost to sight when about 1870 E. T. Newton (later F.R.S.) cut for the use of T. H. Huxley a magnificent series of coal sections, some of which are now in Manchester. Huxley published a few words in a popular essay about the spores visible in these sections, but no serious use was ever made of them. Some twenty years later Potonié and others worked on the micro-structure of some cannel coals and oilshales (which they called 'sapropelic coals') discovering in some of them perfectly preserved remains of algae. Theissen and Jefferies in America began work about 1904, macerating coal chemically and cutting microtome sections of the product. Hickling tried this method in about 1907, but found it unsatisfactory, whilst James Lomax before 1908 had begun to grind coal sections and examine them to try to find some quality associated with liability to spontaneous combustion.

Hickling, long familiar with some of Newton's sections, cut more for himself and got others from Lomax, using them for the studies recorded in a

paper published in January, 1916. In this Hickling states: 'A section of a typical "bitumenous" coal usually appears as an almost uniform deep red or orange mass, with a few darker or paler specks, streaks or lenticles. Here and there, however, the otherwise homogeneous red bands may show, beautifully preserved, the cellular structure of some plant tissue, the walls of the cells appearing as a deeper stain in the red mass.' Fragments of coal showing this structure, though they are a small part of the whole, are very wide spread. 'If any well defined piece of plant tissue is traced out, it is usually found that the visible distinction between "walls" and "contents" gradually dwindles to vanishing point so that the "tissue" merges into homogeneous coal in which it is probable, nevertheless, that tissue structure really exists, though not distinguishable optically. The general impression left, in fact, after extensive examination of this type of coal, is that it mainly consists of such infilled vegetable tissue. A little consideration shows that a conclusion of the highest importance arises from the facts just described. The transparent, visibly homogeneous material constitutes almost the whole of the coal; it must be regarded, therefore, as typical coal substance. But we find this substance filling all the closed cell-cavities of continuous masses of vegetable tissue. It follows, consequently, without possibility of doubt, that the substance originally found its way through the membranous walls of these vegetable cells, and that the coal substance was originally liquid.' This quotation is the earliest statement there is of the nature of the material called commonly vitrain, and of the fundamental process in the formation of bright coal. It would now be accepted as true by all students of coal, and Hickling's later work on coal is largely a great development of its conclusions.

In 1917 Hickling went much further, recognizing that in general each separate lenticle of bright coal represents a single continuous fragment of plant tissue, though its shape seldom has any relation to the natural outlines of the plant structure, and that the material (in Carboniferous coals) was much more commonly cortex than wood. In addition to 'bright coal' there are in ordinary coals 'dull' layers. These are widely variable even in a single seam. They contain minute fragments of the transparent vitrain, strips as little as a thousandth of an inch thick to small elliptical or irregular patches. Pale coloured, ribbon-like toothed strips are cuticles from the epidermis of plants, whilst almost invariably present are the hard coats of spores. Finally there is a matrix filling gaps between the other elements, a mass of most minutely comminuted debris. Masses of 'resin' may be found, and masses of 'mother-of-coal', distinguishable because the cells with opaque brittle walls are empty, or filled by secondary minerals.

After 1918 research on all aspects of coal was organized in Britain in the hands of Drs Stopes and Wheeler, and there has since been a continuous stream of papers on the structure of coals, on the nature of the plants contained in them, on the chemical nature of coal constituents, and indeed on all the problems they present. For example, Hickling, with his student C. E. Marshall, examined the coal which represented the wood of an individual

tree buried isolated in a Coal Measure shale, finding that it was typical vitrain—bright coal—showing very well the structure of the secondary wood, greatly crushed, the walls of the vessels often so much contorted that they seem at first to be broken, but are still usually continuous. Later he went on to deal similarly with *Lepidodendroid* trunks, thus enabling some of the vitrain in sections of ordinary coals to be referred to definite plants. From such evidence it became clear that coals do differ from one another from the time their materials were first brought together and buried under mud. The differences are of kind, and not what is known as rank, that is of percentage carbon content. Hickling contributed to this conclusion by plotting the analysis of more than a thousand coals for carbon against oxygen content. This he did in the simplest possible manner, Ralston having early made more complex plots. Hickling found that all his 'coals' (not, of course, including cannels or oil shales) lie within a narrow belt, which from its beginning in peat to an anthracite of about 90 per cent carbon is straight, and he subsequently showed that the width of the belt represented differences in the initial materials of the coal; the variation along the length depended on later changes, influencing all materials alike. In particular anthracites were the end products of metamorphism, of course of an altogether different grade from that of petrologists. All his subsequent work is devoted to further development of these ideas, and all of it is set out with great lucidity. It is fair to say that Hickling's most abiding interest was the palaeobotanical nature of coal-materials; for something like forty years this constantly recurred in his work, and he acquired, and passed on to his students, a mastery of the preparation of thin sections, studied by transmitted light. On this, as on every other interest, he published far too little. In this work on coal Hickling was associated with a series of colleagues and students, including C. E. Marshall, A. Raistrick, and S. I. Tomkeieff, who have further developed the study of coal essentially on Hickling's lines.

Hickling also performed great public service as an adviser on water supply. His clear understanding of the problems involved, both geological and technical, enabled him to judge possibilities, and his power of clear exposition must have made him an admirable 'geological expert'.

Hickling, taking over a small inactive department, had to do a very great amount of teaching in his early years. But very gradually, as he gained the confidence of his colleagues, more men were appointed, and good students came to him. Thus by 1939 the Department was relatively well equipped, and well staffed, and research was going on actively in many fields. But it remained in a mass of inconvenient rooms on varied levels, and approached by several staircases. The war of 1939 prevented any development, and Hickling retired in 1948. He was long an active member of the Institution of Mining Engineers, and of the Institution of Water Engineers. The Natural History Society of Northumberland, Durham, and Newcastle-upon-Tyne, which has had a long distinguished history—being in fact the last of many such bodies to support a large and important museum, the 'Hancock'—found that the steadily

increasing cost of maintenance of the museum became too burdensome; its existence was assured by an arrangement between the Society and King's College (the Newcastle division of Durham University) made by Hickling, who for some time had been Chairman of the Museum Management Committee.

Hickling was a very good teacher. His lectures, always both logical in development and lucid in presentation, were given in a clear deliberate speech, which made them easy to listen to and absorb. He had a very wide field of interests and knowledge and encouraged any able student to follow his own bent. And he succeeded in enabling them to pass on to other places where their special interests might be more adequately served. He played no games, and had an unathletic appearance, but he could walk extremely fast over the roughest country, decimating any ordinary practical class in a short excursion. He greatly enjoyed driving a car; when tired he often started late at night and would drive perhaps from Newcastle to Birmingham to conduct a practical examination. He drove fast, and apparently inattentively, but in more than thirty years he never had an accident. He seldom came to London, though he served on the Council of the Geological Society from 1919-20 and 1939-41. He was awarded the Murchison Medal in 1934, the G. C. Greenwell Medal of the Institution of Mining Engineers in 1949, and was elected F.R.S. in 1936.

He married in 1910 Gertrude Hermine Geiler, a student of geology in Manchester, who subsequently took a medical qualification there, and had one son and two daughters. Later in life Mrs Hickling did much medical work, in the conduct of clinics, and medical inspection, carrying on the work through a long progressive illness most heroically borne. Shortly before his death Hickling married for the second time Miss Grace Watt, M.A., who survives him.

D. M. S. WATSON

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Harold King.

HAROLD KING

1887-1956

HAROLD KING, who died on 20 February 1956, was brought up in Wales, although he had no Welsh blood in him. His father, Herbert King, who, together with his wife, came of Lancashire farming stock, was a schoolmaster by profession; he had received his training at Carmarthen and had hence become specially interested in Welsh education. Harold, the eldest of four children, was born on 24 February 1887 in the village of Llanengan, Carnarvonshire, where his mother was headmistress of the church school, his father being headmaster of the church school at the neighbouring village of Llanbedrog. Soon after Harold's birth the family moved to Llanystumdwy, where his parents were headteachers of the church school until 1891; in the latter year they moved again, this time to Bangor, the move being dictated by Herbert King's desire to provide the best education for his children; the parents remained head teachers of the St James Church School in Bangor until their retirement in 1923.

It was in this modest and serious-minded environment that Harold King grew to manhood, and the marks of his upbringing remained with him to the end of his life. His earliest education was received at the school where his parents taught; from this he moved to Friars' Grammar School, Bangor, where he spent about five years, and in 1905 he entered University College, Bangor, as the holder of two scholarships. King himself has recorded that at this time he had a general interest in science, but was quite undecided as to which particular branch he would pursue. At the end of his intermediate course he was still undecided, but at this stage he was influenced by the advice of a fellow student to choose chemistry as one of the subjects for his final examination. The advice that was given to King was based on the excellence of the teaching of chemistry by the late K. J. P. Orton who held the chair at Bangor; it was a fortunate circumstance that he accepted this advice, for as he himself said, under the inspiration of Orton's teaching he found chemistry both interesting and easy; his period of indecision was over; he had found the chosen subject for his life's work, and in 1909 he graduated with first class honours.

Orton's influence on King did not cease with the latter's undergraduate career. Graduation was followed by the award of a University of Wales Studentship, and during the two years' tenure of this King served his apprenticeship in research in Orton's laboratory. The experience left a lasting

impression on him and in later years he would speak of Orton always with affection and respect; he even conservatively adhered himself to minor tricks of technique that he had picked up from his teacher.

In 1911 King was awarded an Industrial Bursary by the Royal Commission for the Exhibition of 1851. This type of bursary had been recently instituted by the Commission with the object of encouraging young university graduates in science to enter industry. It seems that the initiative in this project must have come entirely from the Commission; King's experience at least was that, in spite of his good academic record it was extremely difficult to find a suitable opening. Finally he obtained an appointment in the Gas Light and Coke Company's Tar and Ammonia Products Works at Beckton; here he was engaged in the Analytical Laboratory, and gained some useful experience, but it was clear to him that no opportunities for serious research existed, and after six months he moved to a temporary post in the Wellcome Physiological Research Laboratories at Herne Hill.

Although King's stay in these laboratories was again of short duration, the experience was probably the turning point in his scientific career. The laboratories were under the direction of H. H. Dale; King himself worked under the general supervision of George Barger, to whom he had had a letter of introduction from Orton and who had only recently left the Wellcome Laboratories for Goldsmith's College. King worked in immediate collaboration with A. J. Ewins and thus obtained an introduction to research on alkaloids, which later became one of his major interests. What was more important however was that in the Wellcome Laboratories King came for the first time into the environment of biological research. The collaboration between Dale and Barger which has had such fruitful results for physiology and pharmacology had been at its height for some years, and the scientific atmosphere was exhilarating; moreover in these laboratories King had before him an outstanding example of the way in which chemists and biologists can work together to their mutual advantage, and this must have had considerable influence on him in later years.

After six months at Herne Hill King obtained a post in the experimental department of the Wellcome Chemical Works at Dartford; after a period of training in analytical work this appointment brought him into the laboratory of the late F. L. Pyman, with whom he worked on the glycerophosphates and from whom he learnt much in experimental technique. With the coming of war in 1914, King was drawn into the desperate effort that had to be made to establish a synthetic drug industry in this country, which had hitherto depended almost entirely on the products of the German manufacturers; he was transferred temporarily to the manufacturing department and there put in charge of work on the large scale preparation of salicylic acid. Having solved this problem he returned to Pyman's laboratory and succeeded in developing a satisfactory process for the preparation of aspirin with stability equal to that produced by Bayer and at an economic cost. Apart from other practical problems of a similar character, King undertook in Pyman's

laboratory researches on hyoscine which gave him further experience in alkaloidal chemistry.

In 1919 came the appointment which settled the future course of King's scientific career. The Medical Research Committee (as they then were) had decided already before the outbreak of war in 1914 that one of their primary objects should be the establishment of a central institute for laboratory work in medical science; they had purchased a building for this purpose at Hampstead, which had been in use as a hospital, and they had appointed certain key members of staff. The outbreak of war made it impossible to proceed with the plan, since the Hampstead building was required for hospital purposes, and the staff carried out their war-time researches in various institutions. The staff included H. H. Dale and G. Barger under whose general direction King had already worked at Herne Hill and who, during the war, were accommodated in the Lister Institute. At the end of the war Barger was appointed to the chair of Chemistry in Relation to Medicine at the University of Edinburgh and King was offered and accepted a post as chemist on the scientific staff of the Medical Research Committee, with special responsibility for the study of drugs; at the same time the late H. W. Dudley joined the staff to take charge of biochemical research.

From this time until his retirement in 1950 King worked continuously for the Medical Research Council, and in the course of more than thirty years he built up for himself a high international reputation as an organic chemist, at the same time making outstanding contributions to the application of organic chemistry to medicine, both by his own work in the laboratory and by his efforts to keep chemotherapeutic research in the forefront of scientific investigation.

SCIENTIFIC WORK

King's scientific work falls into three main divisions, namely researches on chemotherapy, work on alkaloids of medical interest and the theoretical contribution which he made together with Otto Rosenheim to the chemistry of the steroids.

Chemotherapy

The researches on chemotherapy were taken up in a sense as part of the terms of reference of King's appointment to the scientific staff of the Medical Research Council. The Council were ahead of their time in the emphasis that they laid on the importance of chemistry in medical research, as was shown by the appointments they made to their initial staff; in choosing an organic chemist such as King however they had particularly in mind the need to develop chemotherapy, the power of which had been shown by Ehrlich's discovery of salvarsan.

At the time when King started his work on this subject salvarsan was still the only chemotherapeutic agent of real importance, and his first contribution was a study of the impurities contained in commercial preparations of the

drug which were proving troublesome in its therapeutic application; he succeeded in identifying the principal impurity as 3:3'-diamino-4:4'-dihydroxy-5-sulphoarsenobenzene monohydrochloride; this compound could occur in considerable amounts in some samples of salvarsan and was sufficient to account for the greater part of the sulphur content of these specimens; moreover it was twice as toxic as pure salvarsan, and thus was clearly responsible for some at least of the trouble that had been experienced.

It had been known for a number of years that a variety of organic arsenical compounds were active against experimental trypanosomal infections in animals, and one such compound, phenylglycinamide-*p*-arsonic acid (tryparsamide), had been brought into clinical therapeutic use. Apart from this the earlier observations of the trypanocidal activity of trypan red had led to the development in Germany during the first world war of the very valuable trypanocidal drug Bayer 205 (now known as suramin), a colourless compound having a general similarity in chemical constitution to trypan red. In suramin two aminonaphthalene sulphonic acid residues are linked by a number of arylamide groups. One of the remarkable properties of suramin is the persistence of its therapeutic action, and King conceived the idea that by similar linkage of two *p*-amino-phenylarsonic acid residues he might produce a drug as powerful therapeutically as the available arsenicals but with the additional virtue of a long-lasting effect. In this hope he was disappointed, since the products turned out to be insoluble and devoid of trypanocidal activity; such activity was however observed in the intermediate arylamides of *p*-aminophenylarsonic acid, and King spent much time in the development of this observation, producing large numbers of compounds, many of which showed some degree of trypanocidal activity in experimental infections. No drug of value for the treatment of human disease emerged from these studies however, and so far as the immediate object of the investigation was concerned the work met with the fate all too common in chemotherapeutic research where much industry and careful thought are expended to little apparent purpose.

More rewarding was the later development of King's work on arsenicals, when he took up the study of the thioarsenites formed by the reaction between arsenious oxide derivatives and thiol compounds. This work greatly extended the views advanced by Voegtlin, that the lethal action of arsenical drugs on spirochaetes and trypanosomes was due to interaction with essential thiol groups in the organisms. Furthermore it afforded a reasonable basis for the idea that had been formed that pentavalent arsenical drugs (arsonic acid derivatives) exercised their therapeutic effects only after reduction in the body to the corresponding arsenoxides. Finally the experimental work that King did on the thioarsenites, especially with reference to the reversibility of the reaction between arsenoxides and thiol groups, was a valuable pointer to the important later discovery by Stocken, Thompson and Peters of BAL, the most successful known antidote to arsenical and heavy metal poisoning. As a side issue of his systematic chemotherapeutic work on arsenicals King pub-

lished a valuable series of papers on the constitution of sulpharsphenamine and neoarsphenamine.

Another branch of chemotherapy that interested King over a number of years was antimalarial action. In this research his starting point was the exploration of the effects of modifications in the structure of the cinchona alkaloids on their antiplasmodial action on bird malaria; he reached the conclusion that essential features for biological activity were the presence of a carbinol group in the 4-position of the quinoline ring and the close attachment to this group of a tertiary nitrogen atom, which in the cinchona alkaloids is represented by the nitrogen of the quinuclidine nucleus. Proceeding on this assumption he synthesized a number of analogues of cinchona alkaloids in which the quinuclidine nucleus was replaced by piperidine or its N-substituted derivatives; one such compound, namely 4-(6-methoxyquinolyl)- α -piperidyl carbinol showed in fact a high degree of antiplasmodial activity. Later he was able to show that the same kind of biological activity was to be found in even simpler compounds, such as dialkylaminomethyl-6-methoxy-4-quinoline carbinols and a similar series derived from 6:7-dimethylquinoline.

This considerable body of work again failed to lead directly to a practically useful antimalarial drug. King's contributions however had an important influence on the development of the vast programme of work undertaken in the United States during the war in the search for new synthetic antimalarials.

In 1935 it was shown by von Jancsó and von Jancsó that the compound synthalin (decamethylenediguanidine) which had formerly found transient application as an agent for lowering the blood sugar, had a trypanocidal action which they supposed to be exercised through the host by depriving the parasite of its main source of energy. It was however shown by the late Warrington Yorke that synthalin had a direct action on trypanosomes. This observation was taken up by King who showed, in conjunction with the late E. M. Lourie and Warrington Yorke himself, that long aliphatic straight-chain diguanidines, diisothiureas and more particularly diamidines all showed antitrypanosomal activity; he later discovered that the long aliphatic chain could be replaced by the naphthalene nucleus. A direct result of this work was the development by A. J. Ewins of stilbamidine (4:4'-diamidino-stilbene), which is the most useful known drug for the treatment of kala-azar.

King was associated with his colleague James Walker and with C. H. Andrewes in the discovery of the anti-rickettsial activity of *p*-sulphonamido-benzamidine; this was a most important observation in that it was the first demonstration that rickettsial infections (now for the most part controllable by antibiotic therapy) were susceptible to chemotherapeutic treatment.

Alkaloidal and pharmacological chemistry

Throughout King's scientific career he maintained a continuing interest in the chemistry of alkaloids. Whilst he was in the Wellcome Chemical Labora-

tories, and as the result of his task of recovering *l*-hyoscyne from works residues, he carried out a difficult research on the resolution of hyoscyne and of its components, oscine and tropic acid, which clarified the complicated stereochemistry of this alkaloid. He isolated muscarine from *Amanita muscaria*, showing that it was a substance of greater complexity than had hitherto been supposed. His great contribution to alkaloidal chemistry came however with his work on curare; this is in itself a classical exercise in organic chemistry and it has led to results of far-reaching importance in pharmacology and therapeutics.

The work on the chemical transmission of nervous impulses that had been carried out for many years in the laboratory of Sir Henry Dale at the National Institute for Medical Research had naturally resulted in a strong interest in all substances affecting neuromuscular transmission, and it was this interest that induced King to take up the study of curare. As a source of material he took in the first instance the South American arrow poison tube curare. Earlier work by others had suggested that the active principles of the various native preparations of curare were of the nature of quaternary alkaloids, but no pure compound had been isolated. King succeeded in isolating from tube curare a crystalline quaternary alkaloid (*d*-tubocurarine chloride) and in showing that this was in fact the active principle. In later work he repeated the isolation of tubocurarine chloride from *Chondrodendron tomentosum*, thus proving the botanical origin of the alkaloid.

Having the pure alkaloid in his hands, King proceeded to study its constitution; he was immediately able to show its close relationship to the *bis*benzylisoquinoline tertiary alkaloid bebeerine, demonstrating that *O*-methyl-*d*-tubocurarine chloride and *O*-methyl-bebeerine methochloride were diastereoisomerides; further structural work showed that the orientation of the phenolic groups was in fact the same in the two alkaloids.

In parallel with his work on *d*-tubocurarine chloride King isolated and classified the phenolic alkaloids contained in a second native arrow poison, pot curare, and later he carried out a separation of the alkaloids of *Strychnos toxifera*, the source of the third commonly used arrow poison, calabash curare.

The availability of pure *d*-tubocurarine chloride that resulted from King's work was of immediate medical importance. With the pure alkaloid in hand, dosage could be accurately controlled and safe use could be made of its muscle relaxing properties. In surgical operations the administration of small amounts of *d*-tubocurarine chloride makes it possible to achieve the muscular relaxation required with a much lighter degree of anaesthesia than would otherwise be needed; this is particularly valuable in surgery of the abdomen. *d*-Tubocurarine also found a useful place in the electro-convulsive treatment of certain psychoses, where it could be used to soften the (sometimes dangerous) force of the muscular contractions.

Still greater medical significance attaches however to work that has developed directly from the investigations of curare. An outstanding feature of the molecule of *d*-tubocurarine chloride is the presence of two quaternary

ammonium groups separated from one another by ten atoms. Reflexion on this, together with knowledge already available concerning the pharmacological effects of quaternary ammonium salts, led King to speculate on the possibility of reproducing the effects characteristic of curare by much simpler compounds. He therefore encouraged Dr Eleanor Zaimis, working in his laboratory, to synthesize a series of compounds consisting of straight aliphatic carbon chains to each end of which was attached a trimethylammonium iodide group. Pharmacological test of the member of this series containing ten carbon atoms (decamethonium iodide) revealed in fact that it possessed powerful muscle-relaxing properties. Superficially indeed its action was qualitatively indistinguishable from that of *d*-tubocurarine chloride and, in appropriate dosage, it could be used for similar therapeutic purposes. It should be noted that later physiological analysis has shown that the detailed mode of action of decamethonium is different from that of the alkaloid, but this fact does not detract from the pharmacological interest of the simpler compound.

Much more important however than the discovery of decamethonium was the result of pharmacological test of other members of the series. This investigation by Dr W. D. M. Paton and Dr Zaimis herself revealed a totally unexpected property in the members of the series containing five and six carbon atoms; these compounds proved to be powerful blocking agents for sympathetic ganglia, as the result of which their outstanding physiological effect was to produce a fall of blood pressure. It was soon found that the methonium compounds could, under proper control, be safely used as hypotensive agents in man. The discovery of penta- and hexa-methonium, with the development of other compounds on their model, has in fact resulted in the introduction to medical practice of the first really effective series of drugs for the treatment of hypertension, and has thus produced a major advance in the therapy of a common and hitherto intractable malady.

Before leaving the general subject of pharmacological chemistry mention should be made of King's work on the impurities present in commercial ether and on the chemistry of the deterioration of ether on storage. The results that he obtained had an important influence in the improvement of the quality of ether supplied for anaesthetic purposes.

Steroids

It is paradoxical that although King was first and foremost an experimentalist his name will probably be best remembered for his part in a contribution towards which he did no experimental work whatever. In the early 1930's the accepted formulae for cholesterol and the bile acids were those that had been constructed on the basis of many years of work by the German chemists, principally Windaus and Wieland. These formulae presented several awkward features and elements of doubt, and the doubts were increased by the results of Bernal's X-ray crystallographic studies. The

chemistry of cholesterol and the bile acids had long been a matter of interest to the late Otto Rosenheim who was at that time working in the National Institute for Medical Research, and he and King had many discussions on the subject. In his biographical memoir of Rosenheim, King himself has described how, at the end of one of these discussions, he was inspired with the notion that what was needed to allow a satisfactory interpretation of the observed facts was a fundamental rearrangement of the rings in the accepted structure. It was thus King who had the essential idea, but it is doubtful whether he would himself have developed it further had it not been for Rosenheim's persistence and intense interest in the subject. At any rate, in the event, the two men worked closely together on the problem and the ultimate contribution was truly a joint one.

The proposal for a reconsideration of the sterol and bile acid formulae was made in a short communication published by Rosenheim and King in *Chemistry and Industry* in 1932, and their views were elaborated in several subsequent papers in the same journal. In the first paper a reasoned argument was put forward in support of the fundamental suggestion that the ring structure of the sterols was that of chrysene; it was shown that this system of four fused 6-membered rings would accommodate most of the known facts of the chemistry of cholesterol and the bile acids more easily than the existing formulae, whilst it provided an intrinsically more probable structure in the light of the X-ray crystallographic results.

It is remarkable that in spite of the weight of authority behind the existing formulations the idea put forward by Rosenheim and King was immediately accepted in principle, particularly by those chemists who had done most work in the field. In detail it was shown by Wieland and Dane that a more probable formulation made the fourth ring a 5-membered structure; this however was a minor modification and did not affect the main reasoning of Rosenheim and King, by whom it was accepted.

Important as was the revision of the formulae of cholesterol and the bile acids that resulted from the work of Rosenheim and King the matter did not end there. For into the general group of compounds based in common on the cyclopentenophenanthrene ring structure fell also the sex hormones, the adrenocortical hormones, the steroid saponins, and the heart poisons such as strophanthidin. The chemistry of cholesterol, and with it the chemistry of all these other groups of biologically important compounds, had virtually reached a deadlock when Rosenheim and King put forward their hypothesis; this hypothesis broke the deadlock; in doing so it opened up a vast field to systematic chemical investigation, and it will stand in the history of organic chemistry as one of those few ideas that have revolutionized a whole branch of the subject.

Harold King was one of those rare individuals who are perfectly suited to a lifetime of work in a research institute. Quiet and retiring in disposition he enjoyed the sheltered environment that such an institute can offer and in

which he could spend his days almost uninterruptedly at the laboratory bench which he loved. Many people, after some years of this sort of life, feel the need of additional responsibilities for their own intellectual and moral satisfaction. For King this was not so: he had no interest in teaching or in administration, nor did he apparently need the intellectual stimulus of teaching which many scientific men find so valuable; his inspiration in research came from his own well ordered mind, his persistent scientific curiosity and his contacts with his immediate colleagues, and he remained as fresh in his scientific outlook on the day he retired as he had been when he joined the staff of the Medical Research Council more than thirty years previously.

Although King sought no work outside the laboratory he was ready enough to take on such other tasks for the Medical Research Council as were clearly his duty. He served as chemical secretary of the Council's Chemotherapy Committee for many years, and he performed a great service in the preparation and circulation of *Chemotherapy Abstracts*; these were a most useful source of information, and did much to preserve and to extend interest in chemotherapeutic research in this country. During the second world war he undertook the secretaryship of the Committee on the Synthesis of Penicillin; this was an exacting task, involving him among other things in a visit by air to the United States, which was the first and, apart from a holiday to Ireland, the only occasion on which he left the shores of this country. Much as he enjoyed this visit, it did not inspire him with the desire for further foreign travel, and indeed he remained to the end of his life unashamedly insular in his general outlook.

King was a loyal Fellow of the Chemical Society and for many years an assiduous attender at its meetings. He was a member of the Council of the Chemical Society during 1928-31, and he served for two periods on its Publication Committee. Amongst his other outside activities in science may be mentioned his work for the Pharmacopoeia Commission which he served as a member of the Alkaloids Committee as well as of Chemical Revision Committees.

As a member of the scientific staff of a research institute King was a most valuable man. He was by nature cool and reserved in personal relationships but he was always ready to help a colleague from his own great store of knowledge; moreover he held the loyalty and respect of a succession of junior colleagues, whose names have for the most part not been mentioned in this memoir but to whom he owed much for the progress of his work. His own scientific interests were wide and he was well acquainted with the researches that were going on in other laboratories of the Institute. This enabled him on more than one occasion to take the initiative in promoting collaborative researches; it is evident from what has been said of his scientific work that such collaborations were profitable, and they were happy so long as his colleagues were careful to give full recognition to the part that he had played; on this point he could at times be sensitive.

Apart from motoring and his garden (which he tended with all the neatness

and precision that was characteristic of his laboratory work) King's principal recreation was his scientific hobby of amateur entomology. It was his interest in this subject that determined his choice of district for his retirement; when he left the Institute he settled in a house near Wimborne from which he could easily make collecting expeditions to Cranborne Chase and the surrounding district. King's attitude towards his entomological activities was in curious contrast to that which he adopted towards his professional scientific work; in entomology he remained entirely the observer naturalist, and showed little interest in the experimental approach; he took his natural history seriously however, and by the time of his death he had made much progress with a systematic survey of the Lepidoptera of the area in which he lived; it was pleasant for his friends to observe the happiness and satisfaction that he evidently found in this work.

King retired some years before he needed to, and at a time when it was obvious to his colleagues that his capacity for scientific work was unimpaired. It was characteristic of the man that he should quietly decide, without consultation with anyone else that he had made his contribution and that the time had come when he could reasonably allow himself leisure to pursue his other interests; this decision having been made, he was unmoved by all the efforts that were made to persuade him to continue his research work for a few years longer. The contentment that he achieved in the last six years of his life showed that on this as on other occasions his judgement as to what was best for himself was better than that of others.

The value of King's research was well recognized by his immediate colleagues and by those in other branches of science on which his discoveries impinged; those who were intimately acquainted with his work felt however that the magnitude of his contribution to organic chemistry might well have received wider recognition than it actually did, partly perhaps because he was never a man to push himself forward. He was elected to the Fellowship of the Royal Society in 1933 and had served as a member of the Sectional Committee for Chemistry. In 1941 he was awarded the Hanbury Medal of the Pharmaceutical Society which he greatly prized, as he did the later award of the Addingham Gold Medal of the William Hoffman Wood Trust (Medical) for 1952. His work for the Medical Research Council was recognized by his appointment as C.B.E. in 1950, and this again brought him much satisfaction.

King married, in 1923, Elsie Maud Croft; he is survived by her, together with their only son.

I am much indebted to my colleague Dr James Walker for helpful advice in the preparation of this memoir.

C. R. HARINGTON

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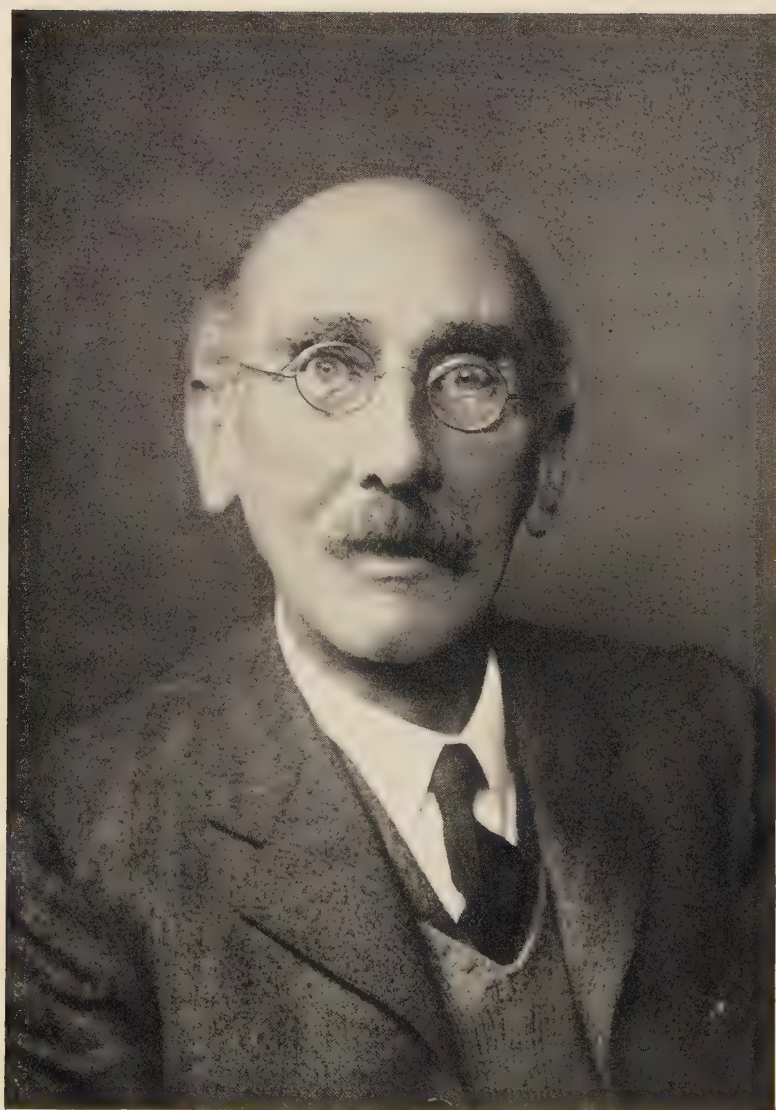
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Charles J. Martin

CHARLES JAMES MARTIN

1866-1955

CHARLES JAMES MARTIN was born at Hackney, in the North East outskirts of London, on 9 January 1866. His parents, Josiah and Elizabeth Mary, lived in one of the Georgian houses with large gardens in Dalston Lane which have since been demolished. Both of them had been married before. Charles acquired one sister by the latest marriage, but was already well provided with older step sisters and step brothers, to one of whom, Howard Rothery a son by his mother's former marriage, he became greatly attached. His father's family came originally from North Walsingham in Norfolk, while his mother, whose maiden name was Lewis, was of Scottish descent, her mother having been a Stuart. In Martin's own words, 'the family into which I was born was a Nonconformist middle class one characteristic of the period, with lots of children, a fading flavour of piety and a small revenue. My father had thirteen children and my mother contributed two more by her first husband. We were not all in residence at one time but it was a "full house". We were well fed and well cared for but there was little money to spare for education and an occasional holiday at the seaside, and all the boys had to start to earn their living at 15 years of age.'

Charles was nominated for the Bluecoat School, then still at Christ's Hospital in the City of London, but he was delicate as a small child and when he became of school age it was decided to send him instead to a boarding school by the sea. He went to Hastings where he remained until he was 15. 'The education', he said, 'was poor, but the opportunities for games and swimming were excellent.'

In 1881 he returned to London to become a junior clerk in the office of the British Life Assurance Company (later to be merged with the Prudential) in which both his father and his step brother Howard were actuaries. He too was placed in the actuarial department, where he worked for about 18 months for a salary of 13s. 6d. a week. Out of this he was able to pay for classes in mathematics which formed part of the training for the profession of actuary, in which his father hoped he would succeed him. Charles decided, however, that he did not wish to continue in that career. He had great leanings towards science and felt the urge to become a doctor, due partly to a youthful admiration for an uncle, Dr Francis Buckell, who had married his father's sister Elizabeth Martin. Dr Buckell had a country practice near Romsey in Hampshire and, as a schoolboy, Charles often spent holidays at Romsey and would

drive with his uncle on his rounds in the pony cart. He was much touched by the glimpses he saw of the patients' gratitude and became fired with that enthusiasm to work for the prevention and cure of disease which never left him.

There was great opposition from the family to this change of career but, through his own determination and with the help and sympathy of his step-brother Howard, he was eventually allowed to begin to study for the Matriculation of the London University, on the understanding that if he passed he could leave his father's office. This project involved attendance at evening classes at Birkbeck College and King's College; it also involved an intensive study of elementary chemistry. Martin used to relate how he bought for twopence a second-hand copy of *A hundred experiments in chemistry for one shilling*. With characteristic determination and industry, some simply contrived apparatus and a few chemicals, he worked through the one hundred experiments in an outhouse in the garden at Hackney. By this simple, if dangerous, expedient Charles as he put it, 'received enlightenment' and afterwards adopted the method in his teaching, being convinced that the first step in learning a science was for the student to *experience* the phenomena. At the age of 17, in the year 1883, he passed his matriculation. Thereafter, according to the agreement with his parents, he took affectionate leave of his home, hired a bed-sitting room in New Bridge Street and became a medical student at St Thomas's Hospital.

It is a curious fact that, at this time, his parents appeared to be quite unaware of the exceptional gifts of brain and character possessed by their youngest son and that little direct encouragement was given to him when a boy in his efforts to acquire further education. It must be remembered that his father was by that time an elderly man in his seventies and had been the father of thirteen children. Charles was the youngest of his sons, so that he might justifiably have been weary of arranging their careers. His mother, however, was a relatively young woman and unusually well educated for that period. In course of time her deep attachment led to sympathy and understanding. She became extremely proud of her distinguished son and they were in close association until her death in 1917, at the age of 87.

At St Thomas's Hospital Martin concentrated his efforts on the study of physiology. Some of the pre-medical studies in the first year were taken at Guy's Hospital and here Martin found a congenial fellow student in Ernest Starling. At the same place Gowland Hopkins was working as an analytical chemist in the newly equipped forensic laboratory. These three young men became lasting friends and Martin would relate how Hopkins, discovering his interest in chemistry, would allow him to 'potter about' in his laboratory and would discuss with him chemical problems in physiology. Soon they were in the habit of lunching together at a neighbouring A.B.C. tea shop and deciding to forego a bun a day to enable them to purchase *Nature* and thus keep abreast of the latest scientific news. Martin recalled also his 'youthful effrontery' in urging Hopkins to abandon his career as an analyst in order

to study medicine and devote his talents to research in physiological chemistry, an ambition which was realized when with a studentship granted in 1888 Hopkins was enabled to start formal medical studies at Guy's Hospital. Martin never lost his interest in chemistry and throughout his long scientific life displayed a sound working knowledge of the growing science of biochemistry.

He took the B.Sc. degree of the University of London with Honours in Physiology in 1886, and was awarded the University Gold Medal and Scholarship worth £50 a year for 2 years. With the proceeds, he went to Leipzig to work under Carl Ludwig, whom he described as his 'physiological hero'. He had been in Leipzig, however, only six months when he was offered the position of Demonstrator in Biology and Physiology and evening Lecturer in Comparative Anatomy at King's College by Professor G. F. Yeo, F.R.S. Professor Yeo had been his examiner in the B.Sc. examination and was obviously impressed by the quality of the candidate. Martin held this post from 1887 to 1891, at first under G. F. Yeo and later under W. D. Halliburton. He managed at the same time to continue his medical education. The duties at King's College did not occupy his whole time but during the term kept him busy most of the day; when these were discharged and in the vacations he was able to work at St Thomas's Hospital. He confessed that his 'medical education was rather muddled owing to the necessity of earning his living'. He qualified as M.R.C.S. and L.S.A. in 1888 and took the M.B. degree of London University in 1890.

First Period in Australia 1891-1903

In 1891 Martin resigned his position as Senior Demonstrator in the Department of Physiology at King's College to accept an invitation from Australia to succeed Almroth Wright as Demonstrator in Physiology at the newly formed Medical School in the University of Sydney.

At that time the salary offered for a junior member of an academic staff was considerably higher in British Universities overseas than in the universities of this country and the change enabled Charles Martin to marry. His marriage to Edith Cross, daughter of Alfred Cross architect of Hastings, took place shortly before the departure for Sydney. It was the result of a long friendship with the Cross family, whose three sons had been pupils with him in the school at Hastings.

On arrival at Sydney, Martin found that Wright had 'less than a dozen students to teach; his teaching had no doubt been unconventional and mostly conversational, but he had founded a research laboratory and had exerted a great influence on both his pupils and colleagues. The results,' Martin confessed, 'I had the advantage to inherit'. The quality of Martin's own teaching in Sydney and later in Melbourne made so lasting a contribution to the progress of medical education in Australia that the phrase 'the Martin spirit', has survived to this day, more than half a century later. It was fully

recognized in the creation in 1951 of the Sir Charles James Martin Fellowships for medical research, which are described later in this memoir.

During the six years he remained in Sydney, Martin made his classic studies on snake venom. With the Australian black snake and tiger snake as his material he succeeded in separating the different poisonous components of their venoms and studied the physiological action of each. Many finely designed series of tests on animals helped him to interpret the action of snake bite in man and to suggest better methods for treatment. For his work on venom he was awarded the medal of the Royal Society of New South Wales.

Martin was a master of apparatus and method and much of his most important work was done with equipment of his own design and construction. By 1896 his gelatin ultra-filter was in full and useful employment in his laboratory. It had enabled him to separate the larger and smaller molecules belonging to the different constituents of snake venom, and also to separate bacterial toxins and anti-toxins. This work was continued after his move to Melbourne in 1897 to become lecturer and later to occupy the chair in the Department of Physiology.

In both Sydney and Melbourne he enjoyed the society of several distinguished colleagues, among them J. T. Wilson the anatomist and later Professor at Cambridge, Edgeworth David the geologist and explorer, J. W. Gregory the geologist, David Orme-Masson the chemist, Baldwin Spencer the versatile anthropologist and Richard Stawell who became one of the most distinguished physicians in Australia. Among Martin's pupils in Australia or among those who were the product of the system of medical education which he did so much to promote were: Thomas Dunhill, Howard Florey, Hamilton Fairley, Macfarlane Burnet, Charles Kellaway, Alan Newton and G. R. Cameron. As Martin himself has said: 'The material was first rate, the quality of the medical student was superior to what I had been accustomed to teach in London. They all meant business and the experimental method of approach came naturally to them and intellectual adventure was congenial.' Nevertheless, his standards were so high that in his first year at Melbourne he found it necessary to fail the whole of the third year medical students in their examination in physiology.

It was already in Sydney that Martin became interested in the anatomy, metabolism and heat regulation of marsupials and of the rare and interesting half-mammals, the monotremes *Ornithorynchus* and *Echidna*, which are indigenous to the Australian continent. His researches in this field provide a fascinating story, showing how the monotremes, in their capacity for heat-regulation, are intermediate in type between the cold-blooded reptiles and the completely homœothermic mammals. By this time Martin was recognized as a distinguished investigator in physiology and in 1901 he was elected to the Fellowship of the Royal Society.

Apart from the stimulus provided by successful teaching and rewarding research, the climate of Australia gave opportunities for a particularly pleasant out-door life. The Martins' daughter, now Mrs Anthony Gibbs, was

born in Melbourne and there were delightful sailing and fishing holidays to be enjoyed by the family. These included trips to Tasmania and sometimes as far afield as New Zealand. Martin often spent his spare time camping and canoeing in more or less unknown country where bush expeditions had the additional spice of exploration, and in his vacations he sometimes took a 'holiday with pay' as locum for an 'up-country' doctor in the bush, where the rounds were made on horseback and where, in addition to his pleasure in riding, he gained useful clinical experience. During term time in the city tennis was his favourite form of exercise.

As a young teacher in Australia Martin already displayed his gift of inspiring devotion in those who worked with him. It was due in part to an engaging simplicity which made him helpful and concerned for all his fellow-workers, whatever their status. For more sophisticated friends he possessed a whimsical charm which, enhanced by an unusual and apt choice of words, made conversation with him delightful and exciting. His way with his humbler friends is shown characteristically by an experience in later life when, after a severe attack of influenza, he spent a lengthy convalescence in a Cornish fishing village. He made friendly contact with two fishermen brothers by helping them to repair the motor of their fishing boat. Later, when one of the brothers fell ill, he himself became the temporary partner of the other, taking his full share in the fishing operations and in marketing the catch in the neighbouring town. He would relate with pride that he earned six pounds in the four weeks that he followed fishing as a trade.

Martin developed a great liking for the Australian people whom he found very congenial, and the attraction was mutual. He remained in close touch with many of his early Australian friends for many years after his return to England, and, as Director of the Lister Institute, he welcomed a constant stream of young Australians who passed through the Institute to gain experience in post-graduate research.

Director of the Lister Institute, 1903-1930

In 1903 Martin returned to England to become the first director of the Lister Institute of Preventive Medicine, then in the early years of its existence. He occupied the position with conspicuous distinction for 27 years until his retirement in 1930 at the age of 65.

At the time of his appointment the Institute had a small scientific staff of about twenty and less than a dozen guest workers. In six years, although the size of the established staff had barely changed, the number of guests had increased to thirty. Many of them were graduate workers, holding scholarships and fellowships from various universities and other countries; they enjoyed the hospitality of the Institute with free use of all its resources. A list of their names reveals how many of those who have since gained distinction in bacteriology, biochemistry or physiology were at some time connected with the Lister Institute and worked in its laboratories.

For direction of research Martin possessed the rare gift of being able to perceive the heart of a problem and decide how it might best be tackled. His advice to the young research worker was to investigate one point at a time and thus to limit his attention to a problem which might reasonably be expected to yield an answer. His guidance as to which aspects might most profitably be studied with the means available was of the greatest value in training the young worker who, as he would express it, was often apt to start away in an attempt to 'investigate the universe'. Direction was nevertheless combined with great freedom. Criticism there certainly was and, if need be, it was blunt and disconcertingly direct, for humility and scientific integrity are qualities not necessarily inherent in the beginner. Sometimes the criticism was hard to accept at first but, with rare exceptions, its high value and friendly intention came to be appreciated. Fools were never suffered gladly and the impatience aroused by insincerity or pomposity often caused resentment.

The years of Martin's directorship are marked by a continuous stream of important original work from the members of his staff and from the guest workers accommodated at the Institute. Very few of the one-thousand odd publications issuing from the Institute at this time will be found to bear Martin's name, although there must have been few that did not owe much to his instigation, inspiration and personal help. He was the most unselfish of directors and seemingly devoid of personal vanity or ambition. His one concern was that the work should be done and done well, by whom it might be published was of minor importance except in so far as a chance might be given to junior workers to achieve some reputation. Preventive medicine was interpreted in a wide sense and the subjects studied varied, as he described it, 'from the epidemiology of plague to the coagulation of proteins by heat or alcohol; from the cause of *purpura haemorrhagica* to the synthesis of vitamin B₁; from the biology of virus infections to the statistical study of cancer incidence; from the mechanics of the disinfection process to the prevention of scurvy and beriberi; from the fermentation of sugar to the domestic habits of the body louse'.

Among the subjects of research on which Martin worked personally during his time at the Lister Institute, the results of which were published under his own name, were, chiefly, the epidemiology of bubonic plague, the chemical and physical properties of proteins and the mechanism of their heat coagulation, the biological and nutritive value of different proteins, the role of vitamins in nutrition and the diseases caused by nutritional deficiencies. He also continued his researches on thermal regulation in man and the problems connected with physical work in hot climates. The results of these and other researches are described under separate headings.

Martin exerted a considerable influence on preventive medicine in a wider sense by the inspiration he gave as member or chairman to various official committees. In 1904, soon after his return to England, he became a member of the Royal Society's Committee on Tropical Diseases, of which he remained a member until 1926; from 1904 to 1911 he served on the Sub-committees

for Malaria, Mediterranean (Malta) Fever and Sleeping Sickness. He had two periods of service on the Council of the Royal Society, from 1909 to 1911 and from 1933 to 1935. From 1904 to 1907 he acted as Chairman to the War Office Committee on Anti-typhoid Inoculation. From 1905 until it was disbanded in 1917, he was a very active member of the Advisory Committee for Investigation of Plague in India and took an important part in its deliberations and researches which involved two visits to India. He was Chairman of the Joint Committee of the Medical Research Council and the 'Field' Newspaper on Canine Distemper from 1923 to 1931, a member of the Medical Research Council from 1926 to 1930 and of its Committee on Biological Standards and Methods of Biological Assay from 1920 to 1923. From 1924 until his retirement from the Lister Institute and departure from England, he acted as Chairman to the Research Committee on Foot and Mouth Disease of the Ministry of Agriculture.

The appointment of Martin as director was followed by an expansion of the Institute's activities in all directions. The Institute was made a school of London University in 1905. Within a few years, in addition to the already existing departments of bacteriology, chemistry, pathological chemistry and of the preparation of vaccines and antitoxic sera, there were added new departments: of protozoology created in 1906 to house Professor E. A. Minchin as University Professor of Protozoology, of statistics in 1909 under the direction of Dr M. Greenwood, and of entomology in 1911 when A. W. Bacot was appointed as the Institute's entomologist.

Work on nutrition at the Lister Institute had its beginning about 1911 when studies on the etiology of beriberi and its connexion with a rice diet were undertaken at the request of Dr Leonard Braddon, a medical officer of the Federated Malay States who was a pioneer in research on beriberi. At the same time Casimir Funk was working as a guest in the department of chemistry and was attempting to separate the anti-beriberi principle from rice polishings and yeast. During the First World War, work on different vitamins and their quantitative distribution in foods was much extended in response to the needs of the fighting forces overseas and the civil population at home. From that time onwards for over 30 years research on vitamins and on other aspects of nutrition occupied members of the staff and many guest workers. It also enjoyed the special interest of the Director as well as his personal collaboration.

In the summer of 1912 Martin made a short visit to Angola in Portuguese South West Africa, in the company of Professor J. W. Gregory who at that time occupied the Chair of Geology at the University of Glasgow. The expedition was sent by the Jewish Territorial Organization under the auspices of the Portuguese Government, to examine and report on the Banguella plateau as a possible territory for a permanent Jewish Settlement. Martin was invited to join the expedition in order to study the public health aspects of the area, with special reference to the presence of the tsetse fly, sleeping sickness and other diseases of man and animals. The expedition

was largely carried out on foot and was after his own heart. He found that the Banguella plateau, 4000 to 6000 ft. above sea level, possessed a fine climate and was remarkably free from tropical diseases. The expedition was followed by no practical action, but it is interesting at this date to reflect upon the difference there might have been in the course of world affairs, had the territory been accepted as the Jewish National Home.

Service in the First World War, 1915-1918

In common with almost every male member of the Institute's staff Martin did military service abroad in the First World War. In the summer of 1915 he joined the medical service of the Imperial Australian Forces, was gazetted Lt.-Colonel and attached to No. 3 Australian General Hospital at Mudros Bay on the island of Lemnos, the base for the Gallipoli Campaign. With his genius for creating something out of nothing he swiftly improvised and equipped an efficient pathological laboratory where the work soon increased to include some service for all the hospitals at Mudros comprising more than 10000 beds.

With the help of a small staff, careful diagnosis was made of the mass of enteric fever cases which were flowing into the hospitals. It was revealed that most of them were due to infection with paratyphoid A and B and only a very small proportion were caused by the typhoid bacillus, against which the troops had previously been vaccinated. The recommendation was therefore made that the Australian troops should in future be inoculated with vaccine containing the paratyphoid as well as the typhoid bacillus. In accepting and acting upon this advice, the Australian Army Medical Corps was in advance of the British Medical Service.

Martin made a study also of the infectious jaundice which was so common in the Dardanelles area that in some units 25 per cent of the strength was affected. In opposition to the accepted opinion that infectious jaundice was a late manifestation of paratyphoid infection, he considered that it was due to a separate specific infection, a conclusion which was abundantly confirmed.

When the Gallipoli Campaign was closed at the end of 1916, the 3rd Australian General Hospital was moved to Egypt and set up at Abbasieh. The move of the pathological laboratory was 'an infernal nuisance' for, as Martin reported, he had just got it 'workable and convenient'. In particular he prized an acetylene generation plant which he had made 'with great pride out of oil drums and odds and ends. It worked beautifully', providing 10 points, which included supplies for lighting, blowpipe and dark ground illumination for microscope work, with everything 'just as convenient as gas'. With five working hours of darkness daily, this was a great boon.

Nevertheless, work was soon in full swing again in the Cairo laboratory with attention turned to the differential diagnosis of the amoebic and bacillary types of dysentery which were infecting the Australian troops. Martin was soon called upon to advise on matters of importance for the military opera-

tions in the Eastern Desert which affected both Australian and British Forces engaged in the Palestine campaign. Cholera had occurred and was a serious menace. Martin's plan, which was adopted, was to set up in the forward area 'diarrhoea camps' with temporary field laboratories attached to each, where cases of suspected serious infection could be diagnosed and treated on the spot instead of being sent back to the base. His scheme was successful in preventing the disaster which could have followed the spread of infection across the Suez Canal into the crowded population of Egypt. The advantages were many. Range of transport for the sufferers was limited, diagnosis was not delayed, the patients were treated promptly and mild cases could be returned to their units in a relatively short time.

Early in 1917 Martin was recalled to England to give advice on cerebrospinal fever. It had occurred among troops arriving from Australia and many of the staff at the Australian Administrative Headquarters were found to be carriers of the Weichselbaum diplococcus. On Martin's advice a Central Pathological Laboratory for the London Command of the Australian Imperial Force was set up at the Lister Institute for diagnosis of the different strains and for control of carriers by methods which he designed.

In a few months, however, Martin was abroad again, this time in France where he was seconded to become Assistant Adviser in Pathology to the British Expeditionary Force, Sir William Leishman being the Principal Adviser. Martin became also a member of the Advisory Council on Medical Matters in France. In spite of his many official duties, research was continued at No. 25 Stationary Hospital in Rouen, with a staff which included Captain (later Sir) Percival Hartley, an old Lister colleague, and some trusted Australian workers. Papers were published on the serological and biochemical properties of different types of dysentery bacillus and on a study of the influenza epidemic which occurred among the staff of the hospital in the summer of 1918. Martin remained at Rouen until he was demobilized at the end of the year.

The extent to which Martin's service to the Australian Army Medical Corps was valued can be gleaned from the many appreciative references to his influence and work contained in the Official History of the Corps written by A. G. Butler. Martin was twice mentioned in dispatches and received the honour of C.M.G. for his contribution to the war effort.

The Lister Institute after the First World War 1919-1930

In spite of the disorganization caused by the First World War and the scattering of the Institute's staff, normal work was quickly resumed when the war was over. Not long before its outbreak, Martin had been appointed Professor of Experimental Pathology of London University and his own laboratory was accordingly renamed the Department of Experimental Pathology. During the absence of its chief it had grown in size by the addition of many women science graduates engaged temporarily to assist in work on

the distribution and properties of vitamins, which was undertaken to supply the urgent needs of the forces and to improve the nutrition of the civil population. After the return of the Director in 1919 nutrition continued to be one of the chief preoccupations of his Department. The small permanent staff became enlarged by the addition of many guest workers who grew in number until about a dozen were usually attached to the Department. Much of the research during this period was concerned with the newly discovered vitamins and was largely of a pioneer nature. It included some of the earliest investigations on the biological action of those already recognized and laid foundations for the study of others to be isolated many years later. Among the investigations which owed much to Martin's inspiration and help, although published under the names of his colleagues, were a series on the sources of vitamins A and D in nature and on the conditions of diet and environment determining their concentration in the milk of cows. In a later period much of the time and work of the Department was devoted to early studies on the separation of the different members of the vitamin B complex.

Research on the nutritive value of proteins in many common foods was carried on with nitrogen-balance experiments on laboratory rats and on lines similar to those made by Martin on himself which are described elsewhere. The discovery by Margaret Boas-Fixsen that the toxicity of dried, raw egg white for young growing rats was apparent when it provided the sole source of protein in the diet, and that the disease produced was due to a deficiency which could be corrected by small amounts of other foods, foreshadowed the discovery of the essential nutrient now known to be biotin.

While intimately concerned with the work on nutrition going forward in his department, Martin was engaged also with his personal experiments on heat regulation during physical work under varying conditions of air temperature and moisture, which are described in a separate section of this memoir.

Towards the end of his time at the Lister Institute Martin, together with Dr Elizabeth Lepper, attacked several physiological and bacteriological problems which were of concern to the clinical pathologist. Simple and reliable micro-methods were evolved whereby changes in the bicarbonate concentration of the blood and pH of the plasma could be followed without undue disturbance to the patient, the amount of blood required being only what could be obtained by a prick of the ear or finger. The effect of changes in temperature upon the pH of blood corpuscles was found to be the same as for plasma, so that from determination of the pH of plasma at room temperature and the known correction to be applied to obtain the pH at 37°C, the pH of the blood circulating in the body at 37°C could be arrived at.

Problems connected with adjusting the reaction of culture media for culture of growing bacteria were also tackled. The interference of salts with the colorimetric method of determining pH with indicators was studied with phenol red and varying concentrations of NaCl. The presence of proteins also

caused aberrations of the colorimetric method. With phenol red and neutral red the colour obtained was decreased by presence of serum albumin and increased by that of serum globulin.

With Dr Lepper, Martin made a study of the principles underlying the usefulness of adding cooked meat muscle to media for cultivating anaerobic bacteria and of the way in which a reducing system was thus produced. Muscle from which the blood had not been washed away before boiling was found to possess a powerful reduction potential and to retain the ability of absorbing oxygen for several weeks, an action due to the presence of haemochromogen derived from the haemoglobin of the flesh. The ability of boiled meat to permit growth of anaerobic bacteria in open vessels was thus explained, as also was the reason why damaged muscles provided an excellent environment for the propagation of the anaerobic organisms that cause gas gangrene.

Second Period in Australia 1931-1933

On 31 December 1930, a few days before his 65th birthday, Martin relinquished his directorship of the Lister Institute, to the distress of the staff and to his own sorrow. Asked whether it was really necessary for directors of the Institute to depart strictly at the statutory age limit, his characteristic reply was that it might not always be desirable but was a necessary precedent to be followed in the case of the first director. For Martin it was a retirement in name only for he had accepted the invitation of the Australian Government's Council of Scientific and Industrial Research to become director of its Division of Animal Nutrition at the University of Adelaide. This Institute, founded in 1927 and directed until his untimely death in 1930 by Professor T. Brailsford Robertson, had been without a leader for many months and much was hoped from the presence, for even a limited period, of an experienced and successful director of research. It was no easy matter for a man of Martin's age to shoulder so exacting and responsible a position. With his usual diligence he made a point, before leaving England, of visiting different veterinary research establishments in the United Kingdom to acquaint himself with the existing state of knowledge about the nutrition of farm animals and on the way to Australia he called at South Africa to visit the Veterinary Institute at Onderstepoort.

Martin's arrival at Adelaide in April 1931 was the beginning of about three years' happy and profitable association with the existing staff. As happened after his earlier sojourn in Australia, his influence has survived the 20 odd years which have elapsed since his departure and is still a living inspiration. The development of the Institute to its present state of importance can be traced to the urge it received in the early thirties under Martin's guidance. Its work, as befitted an Australian concern, was centred upon the nutrition of the sheep and on its protein and mineral requirements, which were of special practical importance because of the deficiencies suspected, and since proved, in the mineral content of some Australian pastures. Certain of these areas

were known to be deficient in phosphorus and Martin applied himself to the study of phosphorus in the nutrition of sheep and lambs. In addition to inspiring investigations designed to reveal and correct the nutritional limitations of the Australian pasturage, he devoted much thought to current husbandry practices and to the pests suffered by wool-bearing sheep. A crude surgical operation made on the breech of the merino sheep had been found to render it less liable to attack by the blow-fly. Martin's simple device of extending the cuts made in the skin of the lamb when 'tailing' was found to decrease greatly the risk of fly strike and this operation is now widely and successfully employed in sheep station practice.

Martin was created Professor of Biochemistry and General Physiology at the University of Adelaide and received the honorary degree of Doctor of Science.

Retirement at Cambridge 1933-1955

By the end of 1933 Martin had settled at Roebuck House, Old Chesterton. Many years earlier, when attending a summer meeting of the Physiological Society at Cambridge, he had slipped away from the hot lecture room in the late afternoon to take a walk and get some fresh air. As he strolled with his colleague J. B. Leathes along the south bank of the river towards the ferry at Old Chesterton, Roebuck House on the opposite bank with its fine lawn stretching down to the water looked so attractive that Martin declared that he should love to end his days in that place. So indeed it came to pass. The interesting old house, its good garden and over-abundant supply of outhouses was entirely to his taste. He took the greatest pleasure in the garden and was able to indulge his hobby for 'tinkering' in a well-equipped workshop.

His retirement was again only nominal as far as scientific work and interests were concerned. He was soon engrossed in an experimental research on *Myxomatosis cuniculi*, undertaken at the instance of the Australian Council of Scientific and Industrial Research 'with a view to the use of the virus in the control of rabbit plagues'. The experiments which were carried out from 1934 to 1936 in the Department of Animal Pathology of Cambridge University, and the practical results which followed are described in detail later in this memoir.

Then followed a couple of years' work on pellagra induced in pigs on maize diets, and on the protein and vitamin requirements of swine. The research was made in collaboration with his colleagues at the Lister Institute and, like that on myxomatosis, enjoyed the hospitality of the Department of Animal Pathology and kept Martin busy until about 1938.

The Division of Nutrition from the Lister Institute at Roebuck House 1939-1945

With the outbreak of War in 1939, a new phase of activity developed in Martin's retirement. At the Lister Institute the Division of Nutrition was occupied largely with experiments on animals. It was unable to continue

working in London and, concerned for the safety of its valuable breeding stock, was searching without much success for a quieter place in which to carry on. The unit had continued in close touch with its former chief and, when his invitation came to take possession of the spare rooms at Roebuck House, it was gratefully accepted. The house had once been an inn and proved to be unusually well adapted for its new vocation. The spacious and well-heated conservatory made a famous animal house, an old coach house affectionately known as the 'mediaeval lab', was converted into a laboratory for rougher chemical work and preparation of food products, the workshop was pressed into the service, and office and library accommodation was provided in the house proper. The necessary arrangements for water, gas and electricity were quickly made under the host's direction and largely with his own hands, in spite of his seventy odd years. A firm bench on which to stand fine balances was hard to arrange in so old a house, but a shelf was finally fixed in the wall of Martin's own room which in turn was invaded also by the evacuees. Under these conditions more than five years were spent in happy and profitable work on vitamins and proteins. For finer chemical work a more suitable home was found at the Biochemical School and for nutritional experiments with large animals at the Department of Animal Pathology. Roebuck House, however, remained the headquarters of the unit. Its host, described by himself as 'chief technical assistant', was indeed all that and a very great deal more, for he was always at hand to share his invaluable experience and wise judgment and frequently to take a not inconsiderable part in the actual experiments.

The energies of the workers were naturally directed to problems connected with the food supply for this country in war conditions and attention was centred largely on the potato as the most important home-produced food and on wheat as the source of our bread. The protein quality and vitamin content of the different portions of the wheat grain were exhaustively studied, and the information gathered and communicated through the Accessory Food Factors Committee of the Medical Research Council and the Lister Institute enabled the authorities to decide which portion of the wheat grain should be included in the flour used for bread making, in order to ensure the most nutritious National Loaf. Studies were made of the supplementary nutritional effect between the proteins of different common foods, for example those of wheat and potato, and between those situated in different portions of the same food. In the wheat grain a supplementary effect was demonstrated between the proteins situated in the outer layers of the grain (bran) and those contained in the endosperm. Martin took an important part in this investigation and allowed himself to be included among the authors of a paper published in 1948 when he was in his 83rd year.

The establishment at Roebuck House constituted an institute-in-miniature with Martin as Director and there is little doubt that he derived much pleasure from the association and from the opportunity thus offered to promote useful scientific work in those critical years. Lady Martin shared

fully in the generous hospitality offered to the Lister workers. Her sitting room which opened directly into the conservatory-animal-house could not, in spite of all efforts, be kept entirely free from evidence of its proximity. Her kindly tolerance, however, never permitted any suggestion that anything was noticed.

Martin's association with Australia was continued in these later years. From 1938 to 1949 he acted as scientific adviser to the International Wool Secretariat, in response to requests for advice on the nature of the research to be undertaken and on the selection of research fellows. Martin's influence can be traced in the decision of the Secretariat to support fundamental research on wool and the accuracy of his judgment was shown in the choice of two future Nobel Prizemen amongst the research fellows appointed. The investigations were carried out at Leeds in the laboratories of the Wool Industries Research Association and in the Biophysics Laboratory of the Department of Textile Science. Even when advanced in his seventies Martin made periodic visits to these Departments which, on his recommendation, received grants from the Secretariat, in addition to the funds needed to maintain the research fellowships. As its Scientific Adviser he was responsible for the financing and promoting of wool research in the United States and all the European countries.

From 1934 to 1946 Martin was chairman of the small governing body which controlled the activities of the Dunn Nutritional Laboratory at Cambridge. He took an active interest in the work and the Director and Staff have testified to their appreciation of his interested and sympathetic guidance.

The last years of Martin's life were saddened by the long illness of Lady Martin followed by her death in 1954 and by his own long period of ill-health which curtailed his physical activities. Unable to take his usual strolls or to tinker in his workshop, he became confined to his room and his failing eyesight prevented much reading. He endured these deprivations with much patience and some alleviation was granted from a new interest he developed in listening to music. With the help of his wireless set he was able to enjoy a wide range of classical music. In spite of physical weakness his scientific interests and curiosity about recent developments remained keen and unimpaired and he was able to enjoy short visits from old colleagues and the scientific discussions which took place at these meetings. He had entered his 90th year when he died.

DETAILS OF FIVE IMPORTANT RESEARCHES

SNAKE VENOM

Soon after arrival in Sydney, Martin became interested in the study of snake venom and its physiological action and in little more than a year had published two papers on the subject. Among its indigenous snakes Australia provided one of the most poisonous known, *Notechis scutatus* the 'tiger snake', whose venom exceeded even that of the King Cobra (*Naja tripudians*), as well

as the 'black snake' (*Notechis pseudechis*), which was less poisonous. Martin's researches, done chiefly with the venoms of these two Australian snakes, have become classic for their scientific importance and valuable for their practical application to the treatment of snake bite in man. In this work, Martin showed the qualities which always characterized his method of approach to scientific problems and his way of solving them. His technical skill was evident in the ingenious method he devised for handling the snakes and collecting the poison; his quantitative bias in the accurate measurement of dosage, calculated always by dry weight, and his careful control of external conditions such as time and temperature. His wide knowledge of physiology was shown in the interpretations he was able to draw from the results of his animal experiments with dogs and rabbits.

The poison of the black snake he found to be of a protein nature and to contain at least three constituents: a neurotoxin causing paralysis, especially of the respiratory system, a cytolytic substance causing destruction of blood cells and of the endothelial lining of the blood vessels, and a blood-coagulating enzyme which caused intravascular clotting. There was evidence that similar principles were present in the venoms of most snakes and that the effect of the bite depended on their relative proportions. In the case of the Australian black snake the blood clotting principle was in preponderance and, after a fatal dose, dogs showed wholesale destruction of blood cells and thrombosis. If, however, the venom was heated to from 82 to 85°C, when a coagulable protein was precipitated, 500 times the usual dose was needed to cause death by intravascular clotting. This result suggested that the blood clotting principle in the venom was a protein which suffered coagulation and destruction by heat. Further, a marked difference was observed in the results following intravascular or subcutaneous inoculation of a minimal fatal dose. Whereas, when the venom was introduced directly into circulation death was due to thrombosis it was caused by paralysis of the respiratory centre if the inoculation was subcutaneous. This difference in effect according to the method of injection was not confined to the venom of Australian snakes but had been observed with other venoms and had given rise to much confusion. Martin concluded that the difference was connected with the rapidity with which the different poisons present in the venom reached the blood stream. This conclusion was confirmed when he succeeded in separating two different poisons in black snake venom by their differing diffusibilities through his gelatin filter.

The Martin gelatin filter, which has played so useful and honourable a part in elucidating the problems of immunity, and incidentally enabled Harden and Young to make the first separation of co-enzyme in 1906, was made as follows. A hot 10 per cent solution of gelatin was poured through a Pasteur Chamberland candle and allowed to set, so that a membrane of gelatin was deposited in its pores. The excess of gelatin was removed later from the inside and outside of the candle by washing with warm water. This filter, when subjected to pressure of from 40 to 50 atmospheres, was

found to hold back albumins, globulins, haemoglobin, other proteins and some dyes and to permit slow passage of albumoses and peptones. Crystalloids passed as freely as water.

The black snake venom, when filtered, was found to have no action on dog's blood *in vitro* and, when injected into a living animal, to cause death not by intravascular blood clotting, but by respiratory paralysis. The *pseudechis* venom therefore contained at least two different poisons, a less diffusible, blood-clotting substance, precipitated by heating to 82°C, and a more diffusible neurotoxin not precipitated by heat. If, therefore, the two poisons as present in the whole venom are injected simultaneously into a connective tissue space, the second, more diffusible one, will reach the circulation more rapidly and the animal may succumb to its effects before the less diffusible constituent has arrived in sufficient concentration to be effective. When, however, the whole venom with its mixture of poisons is introduced directly into the blood stream, they produce their individual effects simultaneously and the animal will succumb to the action of one or the other according to the relative amounts or potency of each and, in the case of the black snake, to the action of the blood clotting poison.

Antibodies to snake poison

The investigations on snake venom were continued into a study of the immunological relations between the venoms and their respective antivenenes. Here Martin came into conflict with Calmette who had made extensive researches on cobra venom and had succeeded in obtaining a satisfactory antitoxin. Calmette maintained that the various constituent poisons in the venoms of different snakes which produced similar physiological effects were identical. Martin, however, was able to show that although the physiological effects were indistinguishable, the antibodies produced in immunization were specific for the poisons of the special species. With his colleague Tidswell, he showed that the serum of a horse vaccinated against the venom of the tiger snake contained a powerful antidote against that poison, but had no effect on the venoms of the three other Australian snakes with similar physiological action.

Issue was also joined with Calmette, Buchner and others who contended that neutralization of bacterial toxins with antitoxins and of snake venoms with antivenenes, could not take place *in vitro* but only *in corpore*, and that some vitalistic operation was involved for which the tissues of a living animal were needed. Martin and Cherry, in a long series of carefully designed tests, showed conclusively that these combinations took place readily *in vitro* in an orderly manner similar to that of a chemical reaction, influenced by temperature, time of contact and relative concentration of toxin and antitoxin. Disregard of these variables, they showed, was the cause of the discrepancy between their results and those of the previous workers.

In these researches also the gelatin ultra-filter played an important part. Since it permitted the passage of the toxin but held back the larger moleculad

antitoxin, the progress of the reaction between them could be studied and its time relations investigated. The difference in diffusibility of toxin and antitoxin explained the different effect observed when they were injected together intravenously or when they were injected subcutaneously, separately but simultaneously, at different parts of the animal's body. In the former case the effect was the same as if neutralization of the toxin had previously been obtained by contact *in vitro*; in the latter, owing to the relative slowness of the antitoxin to reach the blood, 10 to 20 times as much antitoxin was needed to neutralize a given dose of toxin.

The application of these results to the treatment of human snake bite was obvious. It was clear that antivenene should always be injected as quickly as possible into a vein where it would be ready to receive the more rapidly diffusible venom. The value of Martin's researches with snake venom to the science of immunity is tersely and convincingly shown in the contribution he made to the discussion on immunity which took place at the Meeting of the British Medical Association at Oxford in 1904. In his opinion they represented his best scientific work.

THERMAL EXCHANGE AND BODY HEAT REGULATION IN ANIMALS AND MAN

The interest in this subject which Martin developed in his early years in Australia persisted throughout his scientific life and bore much fruit. While at Sydney he became attracted to the study of several species indigenous to the Australian Continent, the marsupials and also the monotremes, those interesting half-mammals, *Echidna* and *Ornithorhynchus* known as the 'duck-billed platypus'. The behaviour of these animals under varying conditions of external temperature was the subject of an important paper communicated to the Royal Society and published in the *Philosophical Transactions* in 1902. It contains a fascinating record and illustrates the unusual quality of the author in the management of the wild animals used, the arrangement of the necessary apparatus, the completeness in the sequence of the experiments and the orderly presentation of the results; it is a model for this type of investigation. The results showed that the reaction of the monotremes to rise of external temperature lay in prompt lowering of their heat production up to a certain point. Above 35 to 37°C, *Echidna* could no longer maintain this response and succumbed. Its normal body temperature of about 28°C rose by about 10°C when that of the environment was raised from 5 to 35°C, and it is therefore classed as the 'lowest in the scale of the warm-blooded animals'. Similar studies with lizards and with the cat and rabbit showed the contrast between the *Echidna's* performance and the passivity of the cold-blooded lizard on the one hand, and the efficient control of body temperature exhibited by the true mammal on the other. Martin concluded that the birth of homœothermic adjustment was achieved by a capacity to regulate heat production but that, in the higher mammals and in man, control depended

to a greater extent on varying the heat loss by evaporation from the skin and by sweating.

The capacity of the human subject to increase his internal heat production in response to cold, as measured by the respiratory exchange, had been much investigated, with the general conclusion that for a clothed man the external temperature could vary between 10 and 25°C (50 and 77°F) without appreciable change in the production of CO₂, but that below 10°C it was increased and shivering occurred. Some workers, however, had concluded otherwise. For example, Leonard Hill observed that after change from a warm laboratory to the roof of the London Hospital, with a cool breeze blowing, the mean oxygen consumption of eight clothed subjects was increased by about 40 per cent although there was no shivering. Martin, on the other hand, in experiments upon four subjects of which he himself was one, found that the oxygen consumption observed in a warm bed after a night's rest was increased by only from 9 to 17 per cent, when a cool breeze was allowed to play upon the naked body, the effect being greater with the thinner subject. When the experiment was extended to the point where shivering appeared imminent, the increase amounted to from 26 to 30 per cent.

In order to investigate further the response of man's heat production to rise of external temperature, Martin took the opportunity of a voyage to Australia in the spring of 1923, to make daily measurement of his own resting basal metabolism, as the temperature changed from temperate limits to tropical heat and returned to temperate conditions once more on meeting the Trade Winds in the Southern Indian Ocean. The values thus obtained for the basal metabolic rate did not vary by more than 12 per cent with early morning temperatures ranging from 62 to 90°F, results which were similar to those obtained later by Gessler in 1925.

The application of his results to the problem of physical work in the tropics captured Martin's interest. Doubtless he was thinking of problems connected with agricultural development by the white man in Queensland and other tropical areas of his beloved Australia, and again he tackled the problem in personal experiments. At the Lister Institute, with characteristic ingenuity, he arranged a small room heated thermostatically with electric radiators and connected with a steam vent for controlling the moisture content of the air. In this chamber he took exercise on a bicycle brake-ergometer, so designed that exercise on it produced a constant rate of work. Wearing only thin pants, he found that if the reading of the wet bulb thermometer did not exceed 88°F he could work without distress at the rate of 1/10 h.p. (the rate usual for a navvy in a working shift) even when the dry bulb registered up to 101°F. The effect of clothing, except possibly of a closely fitting porous garment which became wet and offered no hindrance to the evaporation of sweat, was seriously to interfere with the loss of heat by evaporation, and so cause danger from rise of body temperature. In his own words, 'the obstacle to work in hot climates is, for the European, as much a social as a physiological one. It is clothing. The coolie works with his nice brown body exposed and covered

with sweat, and is jolly, whereas the white man distressfully labours in a hyperthermic condition, straining his heart to work a refrigerating plant which he has rendered inefficient, because his sense of dignity forbids him to expose his skin.'

The importance of atmospheric moisture in influencing loss of body heat was demonstrated with less personal exhaustion by experiments with the instrument which Martin called 'homunculus', a small cylindrical tin filled with water or kerosene and covered with a moistened layer of butter muslin. When the internal temperature was kept constant, at 100°F, by means of a resistance coil carrying an electric current, it was found that with a constant air flow the heat loss from the moist surface, measured by the energy needed to maintain the internal temperature at 100°F, depended only on the reading of the wet bulb thermometer and was unaffected by changes in the dry bulb readings of as much as 12°F, from 85 to 97°F. A more elaborate and fundamental study of the laws governing the heat loss from a sweating body was made with A. J. Canny of Sydney in 1929/1930. The losses by evaporation, convection and radiation from the cylindrical 'homunculus' with its moist cotton covering, was measured under widely differing conditions of air temperature and humidity and in winds of varying velocity. With the use of a simple but lengthy formula, for which values were needed only for the wet and dry bulb atmospheric temperatures, the wind speed and the internal temperature of the cooling body, it was found possible to predict the rate of heat loss from a moist body in any environmental conditions. The absolute quantities obtained were applicable only to the system used, but there was hope that the principles revealed would have useful practical application.

The clothing of the ordinary soldier in hot weather and the protection afforded by his head-gear was also Martin's concern. In the first World War, when he was stationed at Rouen, his sergeant describes how the Colonel on a grilling day started to investigate the state of ventilation in the hats worn by the Australian Army. Holes were punched at different levels in the hat as worn by the Colonel, and standard maximum and minimum thermometers were inserted, while his sergeant read the anemometer and the wet and dry bulb thermometers.

The effects of direct sunshine on heat production were studied with some difficulty, owing to the limitations imposed by the English climate, on the roof of the Lister Institute in the summer of 1928. A special apparatus was designed in which the exposed surface varied from white blotting paper and various clothing materials to an area of skin on the backs of blonde, brunette or coloured human types. The figure for the energy reflected, subtracted from that for the total energy of the direct sunshine, both measured electrically, yielded a value for the fraction absorbed by the exposed surface. It was found that about 60 per cent was absorbed and converted into heat in the naked skin of the ordinary European and about 40 per cent lost by reflection. With a dark-skinned Indian the proportion absorbed was about 80 per cent. The total energy from the sun at an altitude of 45° and with a clear sky is

calculated to be about 4000 calories per minute. When, therefore, about $\frac{3}{4}$ of that amount is absorbed, the heat normally produced in the body is increased about 4-fold. That result, Martin pointed out, would explain how it is that well-pigmented children at Alpine Sanatoria can run about naked in the snow with an air temperature below freezing point, if they are in the sun and there is no wind.

Many of the results of his work on thermal adjustment were incorporated into the Presidential Address and other communications given by Martin to the Sydney Meeting of the Hygiene Section of the Pan-Pacific Congress held in Australia in 1923, when the feasibility of using white labour in the tropics and of work in hot mines were discussed. A general account of his own work on the whole subject, with a critical review of that of others, is presented in the Croonian Lectures delivered to the Royal College of Physicians in June 1930.

THE EPIDEMIOLOGY OF BUBONIC PLAGUE

At the beginning of the present century the appearance and spread of bubonic plague in India were causing great concern to the authorities. The disease, long endemic in Arabia, Siberia and China, had infected Hong-kong and thence had spread rapidly along the eastern trade routes and reached Bombay. In the autumn of 1904 the India Office approached the Lister Institute for advice as to means by which the disease could be controlled. In reply the Director suggested that more precise knowledge of the ways in which the infection was carried would be the best guide for framing measures to prevent its spread. As a result, the Advisory Committee for Investigation of Plague in India was formed, with two members nominated by the Royal Society, one by the India Office and two by the Lister Institute. This small and unconventional body which set to work immediately had the responsibility of appointing investigators, of deciding the plan of campaign, of supervising the work, and of administering the costs.

Four investigators were appointed, two to go from England and two to be selected from the Indian Medical Service, who should carry out the necessary laboratory and field studies. It was decided that, in order to arrange the scheme of work, the Director of the Institute should proceed to India without delay and in March 1905 Martin travelled to Bombay, where he remained for several months. He was followed by Drs Petrie and Rowland from the Institute's staff and they were joined by Captains Lamb and Liston who were seconded for this special research by the Government of India, which arranged for laboratory facilities at the Parel Institute in Bombay.

When Martin returned to London in the autumn of 1905 the research had been organized and a good beginning had been made. The work lasted on the same scale for about three years, at the end of which the results of the experimental and field work left no possible doubt that the origin of bubonic plague in man lay in a rat epizootic and that the infection was conveyed to man from the sick and dying rat by the agency of rat fleas. There was no

evidence that bubonic plague, unlike the pneumonic type, was spread by direct contact from rat to rat or from man to man. The bite of the infected flea was the only means of infection discovered. The full results of the well-conceived and fruitful investigation were published in several special supplements of the *Journal of Hygiene* issued from 1907 to 1914.

It was known that viable plague bacilli were present in the faeces of infected fleas and that the infection could enter the skin of the new host through the wound made by the pricker or be rubbed in by scratching. A further mechanism by which the flea transmits the infection was shown in a brilliant and dangerous series of tests carried out later in the laboratories of the Lister Institute by Martin with the Institute's entomologist, Arthur Bacot. The plague bacilli in the stomach contents of infected fleas sometimes multiplied in the ingested blood to the extent of forming a solid mass of culture that completely blocked the blood sucking apparatus. In subsequent attempts of the hungry insect to suck, regurgitation occurred along the proboscis which was firmly fixed under the skin of the host, so that plague bacilli were successfully introduced into the blood stream.

The application of the results to the control of plague lay obviously in efforts to destroy the rat population in the crowded Indian cities and villages and thus to remove the source of infection from the human inhabitants. This work of prevention had been already well organized during the main period of the Commission's work in Bombay but in January 1908 Martin paid another visit to confer with the authorities on the ways in which this could be continued and to make arrangements for further field work in Bombay City and other towns in the Presidency. Research on the preparation and value of plague vaccines was to be carried on in Bombay and also in the plague laboratory of the Lister Institute which was successfully isolated at Elstree, by Rowland who with Petrie had returned from India in the spring of 1908.

A later investigation of limited scope but of considerable interest followed the appearance, in an East Suffolk village in the autumn of 1910, of cases of an atypical fatal pneumonia, diagnosed as pneumonic plague. A survey of local wild rats made by Martin and Rowland revealed signs of plague infection and it was surmised that plague infected rats, escaped from some ship travelling from the East, had traversed the waterways of our East Coast and carried the infection with the possible aid of cats or other domestic animals. A mysterious illness with several deaths had taken place some months before in a nearby village and this in retrospect was diagnosed as bubonic plague. It was supposed that the first case in the later outbreak had been of that type and that the pneumonia had been a secondary development not uncommon in man and animals suffering from bubonic plague.

These events led to a survey of the fleas infesting rats and other wild mammals in England. Fleas were collected from their chloroformed hosts and tested for their ability to bite man. Of the two types of fleas infesting the field rat *Mus decumanus*, one, *Ceratophyllus fasciatus*, was found to bite the human subject eagerly if hungry. In tropical climates the flea infesting *Mus rattus*

(the black rat) as well as *Mus decumanus*, is usually *Xenopsylla cheopis*, which was acknowledged to be the carrier of bubonic plague to man.

Later work by Martin and Bacot on the influence of climatic conditions on *Xenopsylla cheopis* showed that when away from its host and with external temperature constant, the survival was inversely proportional to the dryness of the air; with the latter constant the length of life was about halved by a rise of temperature of 10°C. The results led to the important conclusion that a range of temperature and air moisture, such as occurs in North India, might determine by 30-fold the longevity of wandering fleas. Such differences in the time available for a plague-infected flea to find a new host might therefore determine the persistence or subsidence of an epidemic.

CHEMICAL AND PHYSICAL PROPERTIES OF PROTEINS

Heat coagulation

Work on the mechanisms involved in the heat coagulation of proteins followed naturally on a study of the laws governing disinfection, made by Harriette Chick under Martin's direction. It had been found that the killing of bacteria by different germicides or by hot water was an orderly time process analogous with the course of a chemical reaction. If external conditions, such as temperature and reaction of the medium, remained constant and the bacterial population was uniform in composition (as for example in a collection of anthrax or other spores in which the more weakly members had been killed previously by heating to from 70 to 80°C) the process of disinfection proceeded in accordance with the Mass Law and the number of organisms killed in unit time was proportional to the concentration of the survivors. The velocity of the process was influenced by rise of temperature in a regular manner in accordance with the law of Arrhenius, the temperature coefficient being generally higher than is usual in most chemical reactions and, for disinfection of some species with hot water, reaching the high figure of over 100 for a 10°C rise. The occurrence of such high temperature coefficients explained the prevalent, though erroneous, conception of a thermal death point for bacteria, since sterilization was observed to occur suddenly when infected solutions were gradually heated.

So mechanistic a view of disinfection evoked much opposition, the prevailing opinion being that the gradual death of a collection of micro-organisms, when exposed to the action of germicides or hot water, was an expression of the differing resistances of the individual microbes, distributed according to the laws governing biological variation. The time relations of the process could not, however, be harmonized with this view and the death of bacteria when exposed, for example, to hot water was better explained as a reaction between the constituent proteins of the bacteria and the water. Since one reagent, the water, was present in great excess, the process would progress as a reaction of the first order, the rate of destruction at any point of time being dependent on the mass of the survivors.

It was to test this theory further that an investigation of the heat coagulation of pure proteins was undertaken. With Harriette Chick as co-worker, a long series of tests were made with crystalline samples of haemoglobin and egg-albumin. The concept of a coagulation temperature for proteins was shown to be as erroneous as that of a thermal death-point for bacteria. The perfect solubility retained by the two proteins after exposure to dry heat at from 110 to 130°C showed the process to be a reaction between the protein and water which was greatly accelerated by rise of temperature. For haemoglobin and egg-albumin the coefficients were, respectively, about 14 and over 600, for a rise of 10°C. With haemoglobin the reaction followed the Mass Law with fair agreement; with egg-albumin the rate fell off as the reaction proceeded. The discrepancy was found to be caused by changes in the medium as acid was withdrawn from it by the coagulum, or in modern terms, to changes in the pH of the solution. Attention was therefore given to the hydrogen ion concentration of the solutions and these tests must be regarded as among the first in which determination of pH was used in biological work. They were achieved with the help of Dr N. Wilsmore, then Lecturer in Physical Chemistry at University College, London, who advised on the making of a suitable apparatus and on how to use it. Compared with the convenient 'box of tricks' provided for the present day student, the apparatus, chiefly home-made, was complicated and clumsy, but it permitted accurate estimation of hydrogen ion concentration, if in a laborious manner. It was before the time when use of buffer solutions was a laboratory commonplace but the maintenance of a constant pH during the process was achieved by working in a saturated solution of a weak acid or weak alkali of low solubility. With excess of boracic acid the acidity was kept constant at pH 3·1; more alkaline solutions were maintained constant at pH of about 9·7 with excess of magnesium oxide. When the pH was thus controlled, coagulation of egg-albumin was found to proceed according to the Mass Law, the rate of change at any time being proportional to the concentration of unaltered protein.

Heat coagulation of proteins had, however, to be regarded as a combination of two separate processes; denaturation through the action of hot water, by which the nature of the colloidal solution was altered, and precipitation which occurred only under favourable conditions depending on the reaction of the medium and the presence of electrolytes. The dispersion of the denatured protein persisted when the conditions were not suitably adjusted for precipitation and was due to the possession by all the particles of a similar electric charge which prevented their coalescence. When the charges were neutralized, as at the iso-electric point in weakly acid solution, precipitation occurred if the dispersion was not too great. The sign of the electric charge carried by the dispersed particles could be determined under the microscope with dark-ground illumination, in a primitive home-made cell through which an electric current was passed. With the help of these primitive observations on cataphoresis, the effect of different electrolytes in conferring a negative or positive charge, and thus assisting precipitation or causing dispersion, was

found to be connected with the charge carried by the more potent ion of the salt. Thus lanthanum nitrate was found to be most potent in assisting precipitation of denatured protein dispersed by excess of alkali, and sodium citrate when the dispersion was due to excess of acid.

Salting-out of proteins

The study of antitoxins was an important part of the work of the Institute, especially for the scientific staff attached to the Serum Department at Elstree. Among other subjects investigated was the purification of antisera. Great efforts were made to achieve separation of the protein fraction to which the antibody was attached so that in its clinical use the amount of foreign protein injected might be as small as possible. Salting-out with various high concentrations of sodium chloride and ammonium sulphate was the method then used.

A series of quantitative experiments with pure crystalline egg-albumin showed how the amount precipitated was conditioned by the concentration of the salt, the concentration of the protein, the hydrogen ion concentration and the temperature. Precipitation of the protein occurred with a separation of the system into two phases, a protein-rich phase which was precipitated and a watery phase which remained in solution. Water, salt and protein were present in both phases but the proportions were different and with appropriate conditions only a trace of the protein remained in solution. The process was found to be extremely sensitive to changes of hydrogen ion concentration in the neighbourhood of the iso-electric point, where slight changes increased the amount precipitated from a negligible quantity to almost the whole of the protein.

The elucidation of the principles involved in these processes roused Martin's interest and imagination and, with Harriette Chick, several investigations were made to determine the physical state in which the different protein fractions existed in colloidal solution in serum and in particular the degree to which their molecules were associated with water. A course of lectures on colloids was at that time being given by Dr Emil Hatschek at the Sir John Cass Institute and on one night a week Martin made the journey to Aldgate to attend the lecture and do practical work under Hatschek's direction.

Studies of the viscosity of colloidal solutions of the purified proteins, in varying concentrations and at different temperatures, proved fruitful. From viscosity tests on thick emulsions of oil and water Hatschek had derived a simple formula which gave an expression for phase-ratio in terms of viscosity and with which it was possible to calculate the relative volumes of the protein (dispersed) phase and of the water (continuous) phase, in the more concentrated solutions of protein employed. The values calculated for the volume occupied by 1g of the various proteins when in solution proved to be satisfactorily constant for each protein and varied from 9.3 ml. for sodium-caseinogenate to 2.3 ml. for egg-albumin. The solution volume of the protein, reckoned from the density of its solution, being taken as 0.7 ml. the volume of water in mls taken up by 1g of the protein in the formation of

colloidal solution at 25°C, was found to be 8·6 for sodium-caseinogenate, 1·6 for egg-albumin, 2·1 for serum albumin, 3·8 for 'pseudoglobulin' (β -globulin) and 5·8 for 'euglobulin' (γ -globulin). These results throw an interesting light upon the sequence followed in precipitation of serum proteins by neutral salts. The eu- or γ -globulin, as the protein needing most water for its colloidal solution, was driven out first as water was withdrawn by the salt; a higher concentration of salt was needed to throw out the pseudo- or β -globulin, while the albumin which appropriates relatively little water was the last to be precipitated.

NUTRITION

Martin's interest in the 'newer knowledge of nutrition' was aroused about 1909 by a correspondence with his old friend Leonard Braddon who was a medical officer in the Federated Malay States. The subject was the causation of beriberi, a disease which at the time had reached alarming proportions in that country. Although the symptoms of the disease were strongly suggestive of an infection by some slow acting parasite, Braddon, on the basis of many years' study, had reached the conclusion that the malady was nutritional in origin and due to the consumption of a diet composed too exclusively of machine-milled polished rice. The idea that beriberi might be due to defective nutrition was so much opposed to current medical ideas that Braddon met with no success in his efforts over several years to get unmilled or parboiled rice substituted for polished rice in Government controlled institutions such as asylums and jails. He had observed that beriberi did not affect the Tamil coolies who ate their rice in the parboiled or 'cured' state, whereas the Chinese workers who consumed machine-milled white rice were frequent victims. Christian Eijkman had previously come to a similar conclusion as to the connexion of beriberi with white polished rice in the Dutch Indies, but his researches had been published in local journals in the Dutch language and had been unknown to Braddon and other British investigators.

Braddon's appeal for help in further investigations met with a ready response from Martin, who recognized nutrition to be an appropriate subject for study in an institute devoted to preventive medicine. From that time for over 30 years its importance continued to be recognized. Martin invited Braddon to work at Chelsea and with E. A. Cooper, who held a Beit Fellowship, he repeated and extended the work of Eijkman and Grijns on polyneuritis of birds, which was accepted as the analogue of human beriberi. The first experiment on vitamins to be made at the Lister Institute in 1910 showed the presence in parboiled rice of the protective substance, and that it was soluble and could be dissolved out by soaking in cold water. This result explained the immunity from beriberi of the Malayan Tamil coolies. A quantitative study, based on determination of the minimum protective dose for birds fed on polished rice, was made of the distribution of the protective substance in rice polishings, yeast and many common foods, and the principle was established that the size of the dose was related to the magnitude

of the total calorie intake and more especially to the proportion of the calories provided by carbohydrate. At the same time Casimir Funk, at work in the Biochemical Department of the Institute, was attempting to isolate the protective substance from rice polishings and yeast. Although he obtained highly potent concentrates, the antineuritic vitamin eluded him and his final product turned out to be not thiamine but nicotinic acid.

Martin followed these researches closely with help and advice. Not long afterwards, in 1915, when attached to the 3rd Australian Hospital at Lemnos, his experience was utilized in a dramatic manner, for it enabled him to diagnose as beriberi an obscure disease which had appeared in the hospitals in that area and had baffled the army physicians. In this instance beriberi had developed on a diet consisting chiefly of white bread and tinned, sterilized meat. This circumstance gave a new impetus to nutritional work at the Lister Institute which throughout the First World War acted as an important base for the needs of its staff serving abroad. More exhaustive tests were made on the quantitative distribution in foods suitable for transport of the antineuritic vitamin, afterwards known as vitamin B₁. Supplies of dried eggs and dried yeast were dispatched for use in the hospitals of Lemnos and in due, if rather belated, course, a 'soup square' containing enough yeast extract to provide a daily protective dose of the vitamin was issued to troops in the Middle East.

The researches at the Institute were extended to similar studies on the antiscorbutic vitamin in efforts to combat the scurvy which had broken out in the armies in Mesopotamia and also to provide, for infants fed on proprietary foods, substitutes for orange juice.

During the aftermath of the war the Lister workers were stirred to fresh enterprise by reports of the prevalence of nutritional deficiency diseases, scurvy, osteomalacia and rickets, in Central Europe and more especially in Austria. In 1919 Martin arranged for a small mission from the staff to proceed to Vienna where the opportunities for study were found to be unusually favourable. It was realized that severe food scarcity has the somewhat dubious advantage of providing naturally occurring negative controls which in conditions of plenty are not available for human experiment. The mission, composed of women led by Dr Harriette Chick and Dr Elsie Dalyell of Sydney, was sponsored jointly by the Lister Institute and the Medical Research Council. It was soon able to show that the result of laboratory work with animals on the newly discovered vitamins applied equally to the analogous human diseases. The cause of rickets in defective nutrition and its prevention and cure in an identical manner by a fat-soluble principle in food or by the effects of sunlight, were conclusively proved. Martin's warm and continued support was very heartening to those engaged in this work abroad, for it did not escape criticism from authorities who held other views as to the etiology of rickets. A visit to the Vienna Mission made in company with Sir Walter Fletcher provided a special flow of inspiration for the investigations.

Martin's interest turned to the problem of the causation of pellagra known by the work of Goldberger and his colleagues to be nutritional in origin and associated with the consumption of maize as a staple article of diet. Its etiology, however, remained obscure. There was considerable evidence connecting the disease with a qualitative defect in dietary protein, an attractive theory since about half the protein in maize consists of the imperfect protein zein. Martin embarked on a series of experiments on himself, always his subject of preference, and on his colleague Robert Robison, in order to determine the biological values of the proteins contained in different foods. The methods used by Karl Thomas ten years earlier were followed and the nitrogen balance was determined on diets providing protein in the form of wheat or milk. The work had been planned to afford a comparison between these foods and maize, but before experiments on the latter could be made, the not too strong digestions of the two experimenters had broken down by reason of the rigorous nature of the tests. In order to obtain the necessary figure for endogenous nitrogenous expenditure the subjects had to subsist on a nitrogen-free ration for 6 to 7 days, until the nitrogen excretion had reached a steady minimum. The technique of these tests has generally been accepted as a model for this type of work and, although the original aim of the research was not fully achieved, useful and consistent figures were obtained for the daily nitrogenous loss in man due to wear and tear of his tissues. This loss proved to be from 2 to 2.5 g nitrogen daily, or about 0.04 g per kg body weight, in the urine and about 1 g in the faeces. This work, together with the investigations on vitamins carried out by the Lister workers in the previous years, was included in a series of lectures given in Melbourne during Martin's visit in 1923, at the request of the Melbourne Permanent Committee for Post Graduate work.

Many years later, when living in nominal retirement at Cambridge, Martin returned to the challenge of pellagra. For several years the nutritive value of maize had been studied by Harriette Chick and her colleagues in the Division of Nutrition at the Lister Institute, but with little success. No nutritional defects in maize diet could be detected in trials with small laboratory animals such as mice and rats. Another experimental animal was needed. The pig seemed most suitable, as being omnivorous in habit and having digestive arrangements similar to those of man. The regulations of the London County Council prohibited the housing of pigs at Chelsea, but Martin offered 'to look after a few pigs at Cambridge'. The work, carried on at the Department of Animal Pathology from 1936 to 1938, developed into a somewhat extensive investigation of the protein and vitamin requirements of swine, in which Martin took over more than his share of the hard routine work. In addition to the 'pellagra' diet composed chiefly of maize, rations containing wheat as a staple diet were used, as well as synthetic diets of purified ingredients. At this period only vitamin B₁ and riboflavin were obtainable in a pure state and concentrates of other members of the vitamin B complex as well as many necessary purified materials, were prepared at

Chelsea. The very onerous clinical work was, however, shouldered by Martin himself with his usual meticulous efficiency. Sometimes the experiments were carried on into the winter and when his assistant fell ill and he 'had to do all the work', he confessed that feeding pigs in the cold and wet and dark was 'a tough job for an old man'. Nevertheless it was done and no detail omitted.

Pig 'pellagra', different in some ways from the human disease but no less deadly, was consistently developed on the maize diet and prevented and cured by yeast or a protein-free yeast extract. When in 1937 nicotinic acid, abundantly present in yeast, was found by Elvehjem and his co-workers to cure black-tongue in dogs, the acknowledged canine analogue to pellagra in man, it was found to be equally potent for pellagrous pigs. Pig No. 18, a miserable, almost moribund 'runt' with severe dermatitis and paralyzed muscles of its hind quarters, was transformed to a rosy healthy specimen of the 'Large White' pig by addition of a daily ration of nicotinic acid without any other change in diet. She has become, perhaps, the most famous animal in pig literature and her portrait figures in many text books on nutritional and veterinary science.

With the use of synthetic diets, study was made possible of the pig's requirements for members of the vitamin B complex other than vitamin B₁, riboflavin and nicotinic acid. When the concentrates now known to contain pyridoxin were withheld, the pigs developed fits and a microcytic form of anaemia; in absence of 'filtrate factor' now known to supply pantothenic acid, the pigs developed a flaccid palsy of the hind limbs.

Immediately after retirement from the Lister Institute and during his second period of service in Australia the science of nutrition again absorbed Martin's energies. Here the dietary requirements and nutrition of sheep were the natural pre-occupation and research in this direction was originated and furthered.

The spectacular response made by cattle in South Africa to the provision of supplements of bone meal had excited great interest among Australian graziers. However, access to phosphatic licks in regions where the soil was unusually poor in phosphorus had shown a poor response in growth of lambs and production of wool, in comparison with the results obtained when similar pastures were top-dressed with calcium superphosphate. The bones of lambs from the phosphorus-deficient pastures were small, light and porous but without signs of deranged bone formation. It seemed therefore that the defective growth was due not merely to lack of phosphorus but to some additional nutritive stress from which the animals had suffered. Other serious mineral deficiencies, notably of copper and cobalt, are now known to exist in some Australian pastures and a deficiency of digestible protein usually accompanies that of phosphorus under natural grazing conditions. As a first step in exploring the mineral requirements of sheep, Martin, with his colleague A. W. Peirce, made an experimental study of the effect, on growth and wool production, of uncomplicated phosphorus deficiency. The otherwise complete ration contained adequate digestible proteins and vitamins.

The results showed a check in growth but the pathological changes were confined chiefly to the bony tissues, causing osteoporosis and osteomalacia in the sheep and occasionally rickets in the lambs. There was no diminution in the quantity or quality of the wool. The other ill-effects observed in low-phosphorus pastures were therefore to be attributed, at least partly, to an insufficiency of the protein required as a vital need for the nutrition of growing, gestating or lactating and wool-bearing sheep.

MYXOMATOSIS

The research covered the epidemiology as well as the pathology of myxomatosis since one aim of the investigation was to explore the suitability of the virus 'for control of rabbit plagues', the main interest of the work lying in the possibility of benefit to the agricultural economy of Australia. That possibility has been fully realized, although at one time it seemed as if successful transmission to rabbits in the wild state would not be achieved.

The two strains of myxomatosis virus which were used came originally from Dr Aragão of the Oswaldo Cruz Institute in Rio de Janeiro, but one, of greater virulence, had been cultivated for many years by Professor Shope of Princeton, U.S.A. The disease was first observed in 1895 by Sanarelli in laboratory rabbits at the Hygienic Institute at Montevideo and had since spread among rabbit fanciers and to laboratories in other towns in South America and California. The infection was apparently limited specifically to the European rabbit (*Oryctolagus*), and rarely if ever infected the American cotton tail rabbit (*Sylvilagus*) or other wild or domesticated animals. Sanarelli was unable to discover any micro-organisms in the blood or pathological lesions of the sick rabbit and concluded that the disease was caused by an invisible virus.

According to Martin's observations, among the first signs were a discharge from the eyes with severely swollen eyelids, followed by severe swelling of the nose and muzzle, and of the skin and mucous membrane at the anal and genital openings. Sometimes small subcutaneous tumours developed later, chiefly on the ears. The blood and the mucinous fluid from the lesions in the mucous membrane as well as from the broken skin over the tumours were all found to be highly infective. With such an abundance of infective material on the surface of the diseased animal it was likely that infection would spread readily by contact, as proved to be the case if the contact were close enough. Infection by air might also easily occur at a short distance through the sneezing and violent respiratory movements made by the sick animal to overcome nasal obstruction. The malady was almost invariably fatal, death usually occurring in a few days.

Aragão had tried to spread the infection by fleas which had fed on a diseased rabbit but positive results were rarely obtained. Martin observed that the flies, frequent in the animal house in warm weather, were attracted by the superficial lesions on the sick animals and would consume the liquid

and dried exudate when the animal was too ill to protest. Such flies were found to contain the living virus, but the disease spread only rarely from sick to healthy rabbits if their respective cages were more than 6 inches apart. This result made it unlikely that the virus could be disseminated through the air. Close contact between sick and healthy rabbits was, however, found very effective for spreading the infection.

Between 1934 and 1935 a series of trials was made in a large compound, surrounded by double fencing of wire netting, situated in the centre of a paddock grazed by cattle at the Cambridge Institute of Animal Pathology. After a rabbit infected with myxomatosis was introduced among a group of 30 to 50 healthy rabbits already settled in the compound, the start of an epizootic was noticed in from 2 to 3 weeks; it spread slowly at first but rapidly later and the colony was usually wiped out in from 2 to 3 months. In two experiments with wild rabbits there were no survivors out of a total of 54 original inhabitants and 15 families born in the compound.

The experiments just described were followed by field trials on the rabbit infested island of Skokholm, off the coast of Pembrokeshire, where R. M. Lockley the owner was anxious to restore to pasture a barren area of over 200 acres. The rabbit population was calculated to be about 10 000 and, in three successive summers, 1936, 1937 and 1938, rabbits inoculated with the virus were introduced in the hope of starting an effective epizootic. In each instance there was some evidence that myxomatosis had spread and dead rabbits were found lying in the open, but by the following spring there was no trace of the disease or of reduction in the rabbit population. Similar results were obtained from field trials in relatively barren areas of Australia infested with rabbits and it is probable that in some way, in natural conditions, direct contact between sick and healthy animals is not close enough to maintain an epizootic. The sick rabbits were usually found dying on the open ground; they might have left the burrows by instinct when ill or have been driven out by their companions.

The natural spread of the disease in recent years in more fertile districts of Australia and in Sweden, France and England must be attributed to the agency of insects. In Australia mosquitoes are known to have acted as carriers and in England fleas are supposed to be implicated. In the semi-natural conditions of Martin's field experiments the contact between the animals was very close, from 30 to 50 being confined in a burrowed space of 500 square yards. During the experiment he is reported to have spent evenings reading near the compound in order to detect the presence of mosquitoes or gnats which might have been involved in the spread of the infection but he satisfied himself that the spread there was due to contact alone.

Immunological studies

Martin was the only investigator of myxomatosis fortunate enough to possess a few rabbits which had survived an attack. In his hands about 4 per cent from those infected with a strain of low virulence recovered and from over

200 affected with a virus of high virulence there was one survivor. The survivors were immune to further infection with myxomatosis and could breed. They produced 35 young but only two survived. The two, a buck and a doe, with their family, finally succumbed to infection by contact, showing that 'the immunity of recovered animals is not transmitted to their offspring to a sufficient extent to militate against the effectiveness of myxomatosis as an agent for destruction of rabbits' (Martin 1936).

The origin of myxomatosis is something of a mystery. It was first observed in America where it affected only the European rabbit and yet had never been recorded in Europe. In 1931 Shope discovered a fibroma in wild cotton tail rabbits and showed that it was caused by a filterable virus. This virus could infect also the European rabbit, which if it survived, as it often did, became immune to infection by myxomatosis. Martin confirmed that immunity to myxomatosis could be thus established, and was able to demonstrate also the reverse reaction by which his few rabbits that had recovered from myxomatosis proved to be immune to infection by the Shope fibroma. This additional antigenic connexion had not been known previously because no previous worker had possessed an animal which had recovered from myxomatosis.

Shope suggested that the two viruses were originally identical or possessed a common ancestor, and that the fibroma virus might have been developed from the myxoma virus by prolonged passage through the cotton-tail rabbit, by some process similar to that by which the virus of variola is converted to that of vaccinia by passage through the calf. Alternatively, it was possible that the fibroma virus having somehow spread to the domesticated European rabbit in South America, had undergone mutations which conferred upon it an enhanced virulence for the European rabbit and a loss of power to overcome the resistance of the cotton-tail.

A further connexion between the two viruses had been shown by the experiments of Berry and Dedrick, in which the myxoma virus, inactivated by heating to 60°C for 30 minutes, had recovered its capacity to cause myxomatosis in common rabbits if mixed with an emulsion of fibroma virus. Moreover, the resulting myxoma virus retained its virulence in sub-culture. Martin interpreted the results of Berry and Dendrick in the suggestion that the heated myxoma virus, though inactive, was not necessarily dead but had possibly lost some constituent essential for its attack which was supplied to it again by the antigenically related fibroma virus. 'Myxomatosis follows the injection of the mixture, not because the virus of fibroma is transmuted into the virus of myxoma, but because a damaged myxoma virus is rehabilitated by the agency of the virus of fibroma or products of its disintegration' (Martin 1934-1935).

HONOURS

The academic honours granted to Charles Martin included fellowships of King's College, London, and of the Royal College of Physicians as well as of the Royal Society, which awarded him a Royal Medal in 1923. Honorary

degrees were received from the Universities of Sheffield, Dublin, Edinburgh, Adelaide and Cambridge. He was knighted in 1927. He was in his mid-eighties when he received a very unusual honour which probably gave him more pleasure than any other and which he called his 'testimonial'. This was the creation in 1951, by the National Health and Medical Research Council of Australia, of two Sir Charles James Martin Fellowships in Medical Science 'to be awarded periodically to young Australians, to give them overseas experience'. In the illuminated address which accompanied the announcement of the foundation of the fellowships, the Council 'on behalf of the Australian workers in medical science' described the creation of the fellowships as a tribute to his great work as a scientist and teacher and greeted him in these words: 'Your work and teaching in Australian institutions laid a solid foundation to research in this country, and your example and encouragement stimulated its progress during its formative years. Your inspiration still permeates its whole fabric, and you are remembered by Australian workers as one of their most distinguished Masters.' Martin was able to receive a visit from one of the first Sir Charles James Martin Fellows; he came from Melbourne to study in this country from the Walter and Eliza Hall Institute of Medical Research at Melbourne.

In the preparation of this memoir the writer has been helped greatly by Miss E. M. Hume; she is indebted to Mrs Gibbs for facts relating to Sir Charles Martin's early life and to many of his Australian colleagues for information concerning his periods of work in Australia.

HARRIETTE CHICK

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D. W. Nichols

JOHN WILLIAM NICHOLSON

1881-1955

JOHN WILLIAM NICHOLSON, who was born on 1 November 1881, was a great mathematician whose chief interest was in attacking problems arising out of physics and astrophysics. How interesting these problems must have been in the days when he was still a student at Manchester and Cambridge will be appreciated when it is recalled that he must have been just over 19 years of age when Planck communicated his theory of the distribution of energy in the normal spectrum to the German Physical Society (14 December 1900) with its rather startling innovation of the elementary quantum of action. Before that very few could contemplate any modification or expansion of Newton's mechanical principles, which, it was generally believed, were sufficient to account for all physical phenomena; though Kelvin's clouds had already appeared on the horizon.

Nicholson was born at Darlington and was the eldest son of John William Nicholson of Highcliffe, Redcar, Yorks and Alice Emily, daughter of John Kirton of Darlington. He was educated at the Middlesbrough High School where he remained till 1898. Here he showed great aptitude in all subjects, but especially in mathematics and science. As a boy he was very fond of country rambles and was an enthusiastic entomologist. He is said to have made fine collections of butterflies, moths, beetles, etc., and one suspects that his enthusiasm for anything of a mathematical character deprived the world of a great biologist!

From 1898 till 1901 he was a student in the University of Manchester and in residence in Hulme Hall. Here he graduated B.Sc., and later M.Sc. Among his fellow students was another young man of extraordinary brilliance. Arthur Stanley Eddington (later Sir Arthur Eddington, F.R.S.), who became his life-long friend. Together they proceeded to Trinity College, Cambridge.

In 1904 he passed the mathematical tripos as Twelfth Wrangler. He was Isaac Newton Student in 1906, Smith's Prizeman in 1907 and Adams Prize-man twice, in 1913 and in 1917. For some time he was a lecturer in the Cavendish Laboratory and later in the Queen's University, Belfast.

In 1912 he was appointed Professor of Mathematics in the University of London, King's College, while C. G. Barkla was still Wheatstone Professor of Physics. In those days King's College had two professors of mathematics, Nicholson's colleague being S. A. F. White. My earliest recollection of him was when we met at a reception given by the Principal of the University, Sir Henry Miers, in the Imperial Institute where the headquarters of the

University then were. There we began a happy friendship which lasted till the end of his life, though in his later years his tragic illness and the intervening distance made any contact with one another impossible.

He was a splendid teacher and greatly appreciated by his students at King's College, many of whom have told me how greatly they profited by attending his lectures. Like other mathematical geniuses he was apt to get absorbed in some problem when he ought to have been thinking of the lecture he had to deliver and I occasionally received a telephone message from him asking me to apologize to a class, which both he and I taught, for his lateness. One day one of these young men said to me that they did not mind his lateness, since they got so much from him even during half an hour.

Nicholson's published work deals with a great variety of subjects and indeed the appended list of publications is probably incomplete. No doubt his most important contribution to science was the consequence of his study of certain nebular and coronal spectra. It was this work which gave the first impetus to the investigation of the extra-nuclear structure of atoms and which suggested the main features of the advances made later by Bohr and his successors. His earliest publication on the coronal spectrum that is known to me appeared in the *Monthly Notices of the Royal Astronomical Society* in 1910, so that his notion of an atom as consisting of a massive, but small, charged nucleus with planetary electrons could not have been derived from Rutherford's experiments on the scattering of α particles. This picture does indeed appear to have been proposed by Nagaoka (*Phil. Mag.* March 1904) and I myself heard it proposed by H. A. Wilson in his inaugural lecture at King's College, London, in 1906. Nicholson would however appear to have been the earliest to apply such atomic models with success and actually to predict the wave-lengths of spectral lines before they had actually been observed!

He found it possible to ascribe many lines of previously unknown origin in the spectrum of the solar corona to a set of states of atoms of simple electrical constitution, by laying down that the angular momentum of the atom could only have certain discrete values. He was led to this by finding that the energy of his hypothetical atom, protofluorine, in a particular state was almost exactly 25 times what he called Planck's unit, i.e. $25 h\nu$,— ν being the frequency of revolution (*Mon. Not. Roy. Astr. Soc.* **72**, 679). Reflecting about this he writes, 'If therefore the constant, h , of Planck has, as Sommerfeld has suggested, an atomic significance it may mean that the angular momentum of an atom can only rise and fall by discrete amounts when electrons leave or return.' These discrete changes in angular momentum he realized later were integral multiples of $h/2\pi$. Bohr never seemed to like this way of introducing Planck's constant and in his original theory of the hydrogen spectrum, which he ascribed to an atom consisting of a central nucleus and a single electron travelling round it in a circular orbit, he introduced Planck's h by equating the kinetic energy of the electron to $1/2 n h \omega$ (ω being the frequency of revolution of the electron in its orbit). The idea which prompted

this choice is that it is the expression for the *kinetic* energy of one of Planck's oscillators. It is of course equivalent to equating the angular momentum of the revolving electron to $nh/2\pi$.

Nicholson's study of the coronal spectrum led him to the conclusion that the cube roots of the wave-lengths of successive lines should be in arithmetical progression and the already observed lines, $\lambda=3534.5$, $\lambda=4359$ and $\lambda=5303.3$ indicated the common difference of the successive values of $\lambda^{1/3}$ to be 1.103 . This of course suggested a line corresponding to $\lambda^{1/3}$ equal to 18.542 , i.e. a line of wave-length $\lambda=6374.8$. The existence of this line was predicted by Nicholson and some time later a strong line at $\lambda=6374.6$ was discovered by Deslandres and Carrasco in an eclipse expedition. This was all the more remarkable since I believe that this part of the coronal spectrum is, apart from this line, practically empty.

He imagined the atom, to which he ascribed the coronal spectrum, to consist of a central nucleus with five electrons travelling round it on the same circle. He called it protofluorine. It has in fact some resemblance to a fluorine atom since this latter is now known to have five electrons of the 2_2 type (in Bohr's nomenclature). I do not know what atom is now believed to be the source of the coronal spectrum studied by Nicholson. It may be added that he correctly regarded the coronal and nebular spectra which he investigated as what he termed 'resonance' spectra—i.e. spectra excited by some type of intense radiation outside the atom in question.

Nicholson's theory of certain spectra of nebular origin is, I think, even more interesting than that of the coronal spectrum—and it may be stated parenthetically that 'nebular' refers to nebulae within the galaxy. I understand that the spectral lines in which he was interested are now believed to be due to doubly ionized oxygen—ionized by the radiation from a very hot star within or in the neighbourhood of the nebula. Nicholson ascribed them to a hypothetical element which he called nebulium. He thought of it as a charged central nucleus round which *four* electrons revolved on the same circle. It is indeed very like doubly ionized oxygen since we may regard this latter as consisting of a 'nucleus' made up of what is usually called the nucleus together with the very stable system of two *K* electrons, net charge 6 with the four *L* electrons corresponding to the four electrons of Nicholson's nebulium. He was able to give a rational account of the nebular spectra and to predict some of the lines and their wave-lengths before they were actually observed, just as in the case of the coronal spectrum. The line $\lambda=4353$ was predicted in his paper on the spectrum of nebulium (*Mon. Not. Roy. Astr. Soc.* **72**, November 1911).

The discovery, by one of his colleagues, of the quantum conditions, $\oint p_i dq_i = n_i h$, was partly due to Nicholson's angular momentum discovery and especially to his inspiring influence (*Phil. Mag.* June 1915) and I remember the enthusiasm with which he received the suggestion that p_i should be replaced by $\Pi_i = p_i + eA_i$, where A_i is the *i* component of the electromagnetic potential (*Proc. Roy. Soc. A*, **102** (1921)).

Nicholson collaborated with T. R. Merton, now Sir Thomas Merton, F.R.S., then Lecturer in Spectroscopy at King's College, London, in two pieces of spectroscopic work. The earlier of these was on the distribution of intensity in broadened spectral lines. Their method involved the use of a neutral tinted wedge. Quantitative measurements were made on the hydrogen α line by this method. The broadening was caused by condensed discharges through the gas at high pressure. They found it to be symmetrical and concluded that this line had more than one component, since the broadening appeared to be due to electrical resolution.

The other piece of spectroscopic work was concerned with intensity relations in the spectrum of helium. Nicholson's peculiar interest in this later work is indicated in a sentence near the beginning '. . . for the elucidation of the main problems of astrophysics the relative intensities of spectral lines may assume an importance scarcely inferior to that of a precise knowledge of their wave-lengths'. He is thinking, of course, of the physical and especially electrical conditions in celestial bodies and nebulae.

Nicholson became a Fellow of the Royal Society in 1917 and was a member of its Council for some time. He became a Fellow of the Royal Astronomical Society in 1911 and served on its Council also. He belonged to many scientific societies: the London Mathematical Society, the Société de Physique, the Röntgen Society and the Physical Society. He was a President of the Röntgen Society and Vice-President of the Physical Society. He was Chairman of the British Association Committee on Mathematical Tables; a member of the Committee on Radio-Telegraphic Investigations; member of the Council of British Scientific Instruments Research Association and Governor of the Technical Optics Department at the Imperial College of Science.

He left London in 1921 for Oxford where he became Fellow and Tutor of Balliol College and where he was Director of Studies in Mathematics till 1930, when grave ill-health forced him to retire.

In collaboration with Sir Joseph Larmor he edited the *Scientific papers of S. B. McLaren*, 1925, and contributed to the volume, *Problems of modern science*, 1921. He also collaborated with Sir Arthur Schuster in bringing out the third edition of the *Theory of optics* to which chapters have been added dealing in great detail with the quantum theory and its application to those parts of optics which are intimately connected with the structure of atoms.

Nicholson had two sisters, Miss L. M. M. Nicholson and Miss C. I. W. Nicholson and one brother. He married Dr Dorothy Wrinch, D.Sc., Fellow of Girton College in 1922. There was one daughter of the marriage, which was dissolved in 1938. He was an inspiring colleague and a good and unselfish friend who appeared to rejoice in the progress and discoveries of his colleagues just as much as if they were his own.

I shall never forget a meeting of the Royal Astronomical Society to which Nicholson invited Professor Arnold Sommerfeld and myself. Sommerfeld was asked to address the meeting and gave a brief outline of the quantum theory as it was at that time, before the inception of wave-mechanics.

Nicholson was not only an outstanding man of science—he was a good man and a friendly and most helpful colleague. I shall retain a pleasant memory of him for the rest of my life. He died on 3 October 1955.

I wish to express my grateful acknowledgments to all those who have given me information and assistance, especially to Nicholson's sister, Miss Linda M. M. Nicholson, to Sir Thomas Merton, F.R.S., for providing me with copies of the joint publications of Nicholson and himself, and to the Librarian of the Royal Astronomical Society.

WM. WILSON

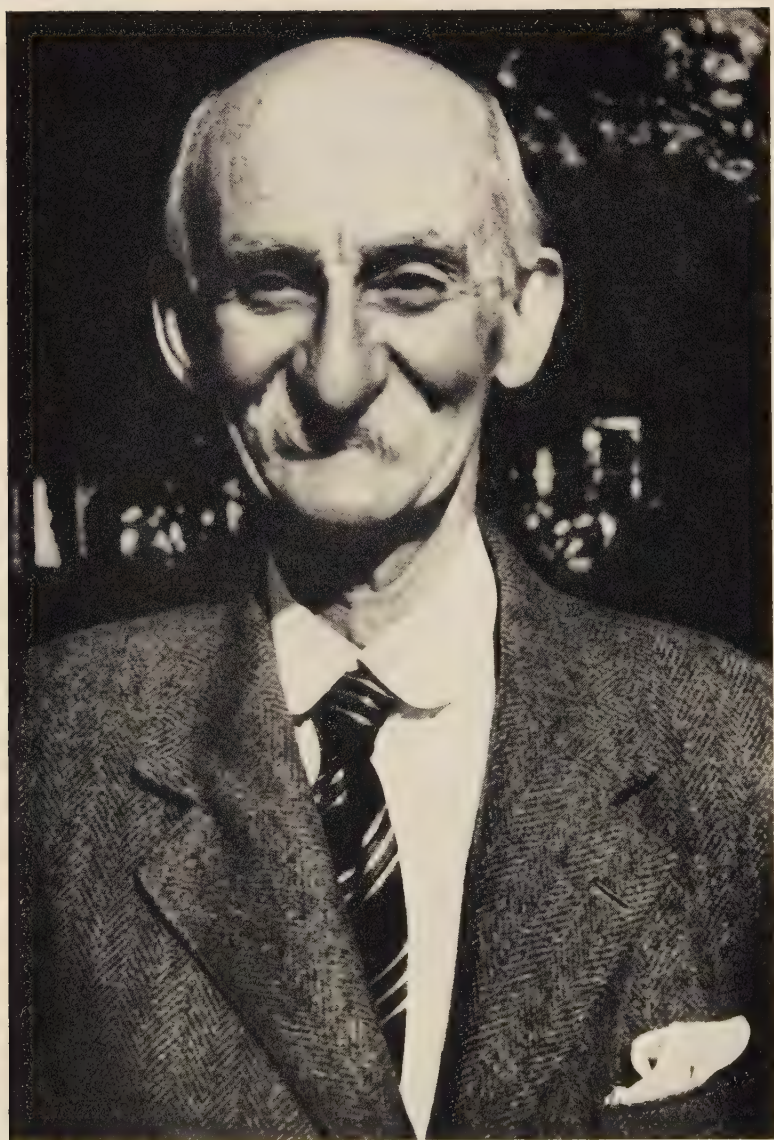
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R. C. G. Perkins -

ROBERT CYRIL LAYTON PERKINS

1866-1955

ROBERT CYRIL LAYTON PERKINS, born on 15 November 1866, died on 29 September 1955, aged 88. A zoologist, principally an entomologist, an outstanding field-worker and taxonomist, his name is inseparably connected with the investigation of the fauna of the Hawaiian islands, besides intensive studies of certain groups of British insects.

Early upbringing and school-days

His love of nature and ardent interest in insects, together with exceptional powers of observation and memory, manifested themselves from the earliest time which he could recall. Yet he had no scientific training whatever at school, nor until he was half-way through his undergraduate years at Oxford.

From his family background several persons stand out, of a type far rarer now (if, indeed, it has not almost vanished); men, largely clergy, who, living in the country for years in the same place, gained an intimate knowledge of the natural history or antiquities of their own districts.

His grandfather, the Rev. Benjamin Robert Perkins, B.C.L., of Lincoln College and Christchurch, Oxford, after working in Buckinghamshire as a parish priest and schoolmaster, was Vicar of Wotton-under-Edge, Gloucestershire, for over 50 years (1829-81). A good classical scholar, he was interested in geology, archaeology and architecture.

Perkins's maternal grandfather was also a clergyman. For his mother was Agnes Martha Beach, daughter of the Rev. Percy Thomas, Rector of Nash, near Pembroke. Her relatives were not, as far as he knew, interested in science. His maternal grandfather was, however, a keen fisherman, and Perkins remembered 'his book of flies tied by himself and of fine workmanship'. Whether this influenced him or not, fishing was Perkins's chief recreation during most of his life.

Though the Rev. B. R. Perkins was not an entomologist, his two sons were keen amateur entomologists from boyhood. Both contributed to entomological magazines and read papers at meetings of societies. The elder son, Vincent Robert Perkins, who lived to be 91, collected insects of all kinds in the Wotton-under-Edge district for over half a century. He wrote for the *Victoria County History of Gloucestershire* lists which remain (unfortunately) still unpublished, while his collections, though scientifically valuable, were sold, after his death, for trivial sums (this information is based on personal letters from Perkins, and his Obituary of his uncle in *The Entomologist's Monthly Magazine*, 1922, **58**, p. 110).

The younger son, the Rev. Charles Mathew Perkins, M.A. (Lincoln College, Oxford), father of our subject, was at different times both schoolmaster and parish priest. Though R. C. L. Perkins was born at Badminton (Glos.), his earliest recollections were of Newland, a village near Coleford (Glos.), whence he was often taken by his father to collect insects in the neighbouring Forest of Dean. He was soon familiar with the butterflies and many of the moths, and had captured a White-letter Hairstreak (*Strymon w-album*), in those days considered a rare butterfly, in his hand, before his fourth birthday. In after years he remembered the exact spots where particular captures were made, such as that of a Black-veined White (*Aporia crataegi*), a butterfly long since extinct in that part of England. Furthermore in his ninth year, when staying with his maternal grandparents near Pembroke in 1875, he noticed the 'most pleasant odour' of the male sex in the Green-veined White (*Pieris napi*), a scent not emitted by the males of allied species; this phenomenon, apparently then unrecorded in Britain, was in 1887 the subject of one of his earliest notes in *The Entomologist's Monthly Magazine*.

Already, before he was seven, on visits to his grandfather at Wotton-under-Edge, he was observing differences between the insects of the Cotswolds and those of Newland. After the Forest of Dean and the Cotswolds, he found much less attractive the district of St Albans, where in 1873 his father succeeded the Rev. T. A. Marshall, a leading entomologist of the day, as Headmaster of the King Edward VI Grammar School. But Perkins's ardour was not damped even by eight years (1877-85) as a boarder at the old Merchant Taylors' School, then still in the City of London. In these grimy surroundings almost the only insects were stray moths from nearby warehouses, and a few species found on lime trees in Charterhouse Square. Later, however, in the parishes of which his father was successively Rector, Perkins found a far more congenial environment: Sopworth (North Wiltshire), Raglan (Monmouthshire) and especially Alderley, near Wotton-under-Edge.

It should be here mentioned that though Perkins was, like many entomologists, first attracted by butterflies and moths, it was not the Lepidoptera, but bees, wasps and other Aculeate Hymenoptera which were his favourite study throughout his life. Possibly his thoughts were turned in that direction by his having, as a child, watched his father's bee-hives at Newland on hot summer days, to give notice of a swarm; or by watching with intense interest a nest of hornets, which his father took from a hole in the garden wall and installed in an upstairs room under a bell-glass, in such a way that the hornets could come and go through an open window. But he did not lose interest in the Lepidoptera.

As to his lack of scientific teaching at school: none was then given at St Alban's Grammar School, which he entered (though under the admittance age of eight) when his father became headmaster. Merchant Taylors' had already a Modern Side, but Perkins's father and both his grandfathers had been classically educated, and he was placed on the Classical Side of the Upper School. Though later, when changing from classics to science in the

middle of his university career, he found himself handicapped by lack of scientific knowledge, he nevertheless appreciated this classical learning. He found pleasure in recalling, in solitary mountain-camps in the Hawaiian Islands, classical poetry learned by heart at school; while the reading and recitation of poetry, Greek, Latin or English, by a school-fellow at Merchant Taylors', now renowned as Dr Gilbert Murray, O.M., made a lasting impression.

University years

In 1885 Perkins won an open Classical Scholarship at Jesus College, Oxford, supplemented by a School Exhibition. For two years he attended only classical lectures. But after hearing a lecture on the colours of insects delivered (in the City of Oxford, not in a University course) by E. B. Poulton (later Sir Edward Poulton, F.R.S.), Fellow and Tutor of his College, Perkins consulted Poulton about the possibility of changing from classics to science. This was arranged but, his school work having been entirely classical, he had to pass preliminary scientific examinations before starting on zoology. Rather to his disappointment the zoological lectures were advanced courses on groups of animals, mostly marine, in which he was not specially interested. Next to insects, birds interested him most. But neither entomology or ornithology were touched; teaching of the former was, in those days, very little developed. But a few lectures on the geographical distribution of vertebrates by P. Chalmers Mitchell (later Sir Peter Chalmers Mitchell, F.R.S.), then a young Demonstrator in Animal Morphology, proved more congenial. However, Perkins graduated in 1889. Subsequently, in 1906, he was awarded the degree of D.Sc. (Oxford).

While at Oxford he joined the then recently resurrected Oxford Natural History Society, of which Poulton was President. Perkins devoted his spare time to collecting and studying the insects of the Oxford district, particularly bees and wasps, the local fauna of which was then almost unrecorded. On these he read a paper to the Society. But apart from Poulton and Professor Arthur Sidgwick (whose lectures on Greek plays he had greatly enjoyed before leaving classics), he met no one specially interested in entomology. Poulton, incidentally, was not yet Hope Professor of Entomology; Perkins often saw his famous predecessor, Professor J. O. Westwood, in the Museum but (to his later regret) never ventured to address Westwood, aged and feeble as the latter then was.

During the later part of his time at Oxford or shortly after (before he left England for the Hawaiian Islands) Perkins, by breeding the caterpillars of various Lepidoptera, experimented on their variation in colour according to change of environment. Several of his observations and opinions were cited by Poulton in *The colours of animals, their meaning and use* (2nd edition, 1890), in the preface to which (p. xii) Poulton was 'especially pleased to speak of the help' received from his former pupil. Unfortunately, a paper submitted by Perkins to Poulton on the course of development of the mark-

ings in the wasp-like parasitic bees of the genus *Nomada* was never published, because the hymenopterist Edward Saunders doubted the correctness of Perkins's conclusions. But the latter, who had dealt with the development of these markings much as those of Lepidoptera were afterwards treated by the late F. A. Dixey, F.R.S., still held, years later, that he was right.

He also sent many British Lepidoptera, including some striking varietal forms, to one of his correspondents, C. G. Barrett, F.E.S., who was engaged in writing his book, *The Lepidoptera of the British Islands* (11 vols., 1893-1907).

The foregoing paragraphs indicate not only the breadth of Perkins's interests, but that his love of the Lepidoptera was not displaced by his having chosen Hymenoptera as his favourite subject. His studies of a third great order of insects to occupy his attention, the Coleoptera, and of several other orders, smaller but not less interesting, were to come later.

Perkins's life-work may be treated under three main heads: (1) investigation of the terrestrial fauna of the Hawaiian Islands, (2) economic (or applied) entomology in those islands and (3) study of British insects, mainly Hymenoptera. But these activities did not follow one another in sequence. He went on studying the Hawaiian collections at intervals while engaged in economic entomology, and even after retirement; while his studies of British Hymenoptera, begun at school, continued, whenever he was in England, as long as his powers lasted. After dealing with these three heads I have tried to assess his systematic work as a whole.

The Hawaiian fauna

After graduating at Oxford, he was engaged for over a year as a private tutor at Dartmouth. He was then chosen, in 1891, by a committee set up in the preceding year by the British Association for the Advancement of Science to investigate the land-fauna of the Hawaiian Islands, in co-operation with a committee of the Royal Society formed for the same purpose. The two committees functioned together as the 'Sandwich Islands Committee' for 22 years under three successive chairmen, Sir William Flower, F.R.S., Professor Alfred Newton, F.R.S., and Dr F. D. Godman, F.R.S. But its Hon. Secretary throughout was Dr David Sharp, F.R.S., Curator of the insect-collections at Cambridge,* where the vast Hawaiian material, while being sorted and sent out for study, was stored in the Museum of Zoology.

Perkins, already associated with Poulton since his undergraduate days at Oxford, now also became acquainted with two notable Cambridge personalities. Under the initial guidance of the first, David Sharp, he added Coleoptera to the orders of insects in which he specialized. During periods spent at Cambridge he worked out, besides his favourite Aculeate Hymenoptera, several families of Hawaiian beetles and some other orders, as set forth below. He occasionally produced papers on Coleoptera from the

* Dr Sharp retired from his curatorship and left Cambridge early in 1909, but remained Secretary of the Committee till it was wound up in 1913.



Above: coastal scenery in the island of Kauai, showing the deep erosion of wet, seaward exposures. The ridge in the background rises above 4000 feet on the right; that in the foreground shows the gentle slopes of ancient lava-flows.

Below: forest-covered ridges, Halawa Valley, in the island of Molokai.

Both from E. C. Zimmerman, *Insects of Hawaii*, 1 (1948).

Hawaiian and other Pacific islands till as recently as 1938. Moreover a close friendship between him and David Sharp lasted till the latter's death in 1922.

The other personality, an older man than David Sharp, was Alfred Newton, Professor of Zoology at Cambridge since the establishment of the Chair in 1866. Besides being Chairman of the Sandwich Islands Committee, he held the Professorship till his death in 1907 (though latterly through infirmity he no longer gave lectures). As an ornithologist Newton, being particularly interested in the study of Hawaiian birds, afforded Perkins 'all possible help'; for the veteran Professor was most kind to younger men (as the writer, having known him in the last year of his life, can testify).

Perkins worked for the Committee for almost ten years (1892-1901). Nearly six years were spent in the islands, while the remainder were accounted for by a few months' visit to England in 1894-5, and a visit of about two years (1898-9) to study the material collected; he was also absent from the islands on work in California for some time in 1897. After his service with the Committee, while engaged in economic entomological work in Honolulu, he collected still more material. The magnitude of his achievement, and the arduous conditions in the field, may be better gauged if the significance of the whole enterprise be briefly recalled.

Darwin had called attention to the peculiarities of the faunas and floras of oceanic islands in *The origin of species* (chap. 13) and Wallace had classified islands in categories, mainly 'Oceanic' and 'Continental', in *Island life* (1st edition 1880). Works on the floras and parts of the faunas of a number of island-groups had appeared; e.g. T. V. Wollaston's studies of the Coleoptera of the various Atlantic islands, published during the years 1854 to 1877.

In the oceanic Hawaiian Islands, the highly peculiar endemic plants were already fairly well known from the fine general *Flora* of W. Hillebrand (1888). The birds and land-shells had been investigated to a point, and enough was known of some of the insects to indicate how valuable a comprehensive investigation of the land-fauna would be.

These islands were admirably suited for such an investigation. Entirely volcanic (with volcanoes still active in Hawaii itself, the largest island), they are isolated in mid-Pacific, over 2000 miles from California, the nearest continental land. Apart from low coral-islands, the closest of which (Johnston Island) is 600 miles away, the nearest volcanic archipelago, the Marquesas, lies some 2000 miles distant. Surrounded as the Hawaiian Islands are by ocean over 15000 feet deep, their formation points to their never having been connected with any land-mass, while depths of 6000 to 9000 feet in the channels between the individual islands indicate that these have probably been separate at least while the fauna has been in process of evolution. The height of the mountains, reaching from over 3000 to above 5000 feet in the smaller islands, and attaining 10000 feet in Maui and nearly 13800 feet in Hawaii, and their erosion into valleys separated by extremely steep ridges, favour the development of locally endemic forms (compare plate facing this page).

The land-fauna was found to bear out completely the oceanic character of the archipelago. The very numerous endemic species evidently arose from comparatively few ancestral immigrants which reached the islands, probably at long intervals in the past, through natural means of dispersal. Only this will account for the entire absence of whole groups of organisms generally distributed over the earth, many members of which would appear no less able to traverse wide spaces of ocean than those which have succeeded in doing so. On the other hand, among the descendants of the ancestral immigrants are remarkable complexes of closely allied endemic species, or even groups of closely related endemic genera. Geographical isolation, above all, has resulted in an extraordinary multiplication of specific forms, with a high degree of local endemism, for many species are restricted to single islands or even to one part of an island.

The above and other important conclusions were set forth at length by Perkins in 1913 in his outstanding 'Introduction: being a review of the land-fauna of Hawaiia'. This was the culmination of *Fauna Hawaiiensis*, a monumental three-volume work edited by David Sharp, and published for the Committee by the Cambridge University Press in parts (1899-1913). Perkins's own contributions, previous to his 'Introduction', comprised systematic accounts of the vertebrates (consisting almost entirely of birds) and groups of insects belonging to several orders. These works are listed in the bibliography. Besides his work on the Hymenoptera, his account of the Coleopterous family Proterhinidae is specially noteworthy. This family, having a remarkable geographical distribution in the islands of the Pacific and elsewhere, is represented in the Hawaiian Islands by over 160 endemic species of *Proterhinus*. It was fitting that when, some years after he had left the Hawaiian Islands, representatives of this genus were discovered by other collectors in the Phoenix and Marquesas archipelagoes, far to the south and south-east, he described the new species.

Though the birds had been previously studied by Scott B. Wilson and others, Perkins devoted considerable time to observing their habits and collecting specimens, producing in 1903 a most interesting account in *Fauna Hawaiiensis* (1, 368-465). He there discussed the high degree of endemism, particularly in the forest-haunting Passerine birds, most of which are assigned to an endemic family, the Drepanididae. He considered that, allowing for their much smaller numbers compared with insects, evolution had followed the same lines; in fact, in these Drepanididae it has gone even further, for extreme specialization of structure in accordance with habits has necessitated the erection of a number of genera, each containing only few species, though all were probably derived from very few original immigrant species. Also specialization carried to these lengths has resulted in many birds becoming extinct or very rare, since the destruction of the lower forests and other changes due to human agency—a regrettable fact, for these birds were harmless and very beautiful, including some of the few brilliantly coloured forms in the endemic fauna.

He also devoted what time he could to collecting invertebrate animals other than insects. But he realized that he had only dealt with these 'very imperfectly' and that, particularly in the case of land-snails, much more has been done by other specialists. He recognized in these groups, however, the same general characteristics of the fauna, exuberant development of species and genera, apparently derived from few ancestral immigrants, coupled with the absence of whole families widely distributed elsewhere.

Perkins's greatest success as a collector lay in the number of insects which he discovered. The inconspicuousness of the endemic insects—for, excepting a single handsome butterfly and a few other insects, almost all are small or minute and few are even brightly coloured—together with their hidden modes of life led earlier observers to imagine that both species and individuals are much rarer than in reality. These characteristics, incidentally, are shared by the endemic faunas of other oceanic islands.

As to the supposed paucity of species, the known insect-fauna was raised by Perkins's labours to over 3300, more than 2700 of which are endemic. Estimating that many had already vanished, he summarized the causes of their extinction, directly or indirectly through human agency. Realizing that the various orders were still very unequally known, he reckoned that at most half the existing species had been brought to light. Perhaps he underestimated the results of his work. But he wrote of his 'Introduction', in its opening sentence, that 'its chief use may be as a guide to future and more special research'. Among those who have taken up this challenge, Dr E. C. Zimmerman states that the total number of known Hawaiian insects now approaches 6000, of which over 3700 are endemic.

As to the apparent fewness of individuals, even the Rev. T. Blackburn, who had discovered a number of the small endemic beetles some years before Perkins's arrival, could rarely find more than very few specimens. Blackburn's remark that, during a morning on the mountains he would collect only two or three specimens, and see literally nothing else except a few common species, evoked from Wallace the comment that 'to those who have collected in any tropical or even temperate country on or near a continent, this poverty of insect life must seem almost incredible' (*Island life*, 3rd edition, 1902, p. 319). Yet Perkins obtained very numerous examples of many of these species. This was due, no doubt, not only to his natural genius, but to his careful observation of their biology. Very many of these beetles are flightless, sluggish and well hidden; even those obtainable by indiscriminate methods were almost all the subject of special search, because he wished to know about their habits, variation and association with particular plants ('Introduction', pp. xxxiv-v). Regarding flying insects, especially the very swift smaller Hymenoptera, his wonderful precision of sight and swiftness of hand played a great part, not only in the Hawaiian Islands but wherever he collected these insects.

Though his employment to investigate the Hawaiian fauna necessitated primarily the collecting of material and description of species, he was too

broadminded to regard the obtaining of new species as an end in itself. He closed his Presidential Address to the Hawaiian Entomological Society (1906) on 'The insects of Tantalus' (a mountain near Honolulu) with a plea for inquiry into the life-histories of native insects already described, rather than for the bare discovery of further new species.

Members of highly equipped modern expeditions may well contrast the arduous conditions under which Perkins worked, which he related briefly in his 'Introduction' (pp. xxxvi sqq.). Most of his time was spent in the zone of continuous forest extending upwards from the cleared lower slopes. He could usually get a horse or mule to carry his tent, provisions and other equipment to the edge of the forest, at altitudes between about 1200 and 3000 feet. Beyond that point he climbed on foot into the untrodden, rain-soaked forest, carrying everything packed on his back, including sometimes a small stove and a supply of oil, as the continuous heavy rain often made it difficult to start a fire after a long day's collecting. For in the wettest areas on the windward side of the islands, between 1200 and over 5000 feet above the sea, the annual rainfall may average anything from 200 to over 500 inches, and few days are ever fine throughout.

He was generally unaccompanied. Native islanders in those days feared to pass the night in the forest, while their presence would also have meant extra baggage. Sometimes he saw no human being for weeks on end, being usually out collecting when an occasional messenger came up to his tent with letters.

His tent was so small and low that birds had to be skinned, and the more delicate insects pinned, as he sat or lay on the ground. Many of the smallest and most fragile moths, collected in very wet and windy places, whence they could not be carried back to camp without damaging themselves, had to be killed and pinned on the spot, often under an umbrella.

Though Perkins was short and slightly built, he had great powers of endurance. Nevertheless his hardships, particularly the frequent impossibility of changing damp clothing, may have permanently impaired his health, for he suffered many illnesses in later years. In comparison, as the writer knows, even a single scientist on *safari* in remote African mountains, though obliged to undertake fatiguing rides and walks and to bear abrupt changes of climate, and though needing an armed escort, has at least servants and camp furniture, above all a dry bed.

The vast material, totalling perhaps 100000 specimens, was specially packed and shipped in lots from Honolulu to England, with scarcely a specimen damaged. As stated, it was all eventually studied and the new forms were described in *Fauna Hawaiensis*. The result was a great extension of scientific knowledge, not only of the Hawaiian fauna in itself, but of the general nature of the faunas of oceanic islands. Moreover, the British Museum (Natural History) was enriched by a first set of the material, including almost all the type-specimens of forms new to science. Other sets were presented to

the specialists who respectively worked out the groups, and to the Bernice Pauahi Bishop Museum at Honolulu, the Trustees of which gave substantial financial aid to the Committee during the later years of the investigation.

Economic entomology in the Hawaiian Islands

Though Perkins's work for the Sandwich Islands Committee closed at the end of 1901, he collected in 1902 and 1903 a great many more native insects, besides carrying on extensive investigations into the biology of the parasitic Hymenoptera and the native leaf-hoppers (Homoptera). After 1903 his opportunities for field-work were much rarer. But he still named large numbers of specimens, and completed his contributions to *Fauna Hawaiiensis*.

He was, however, mainly engaged in economic (or applied) entomology. The United States, having annexed the islands in 1898 and constituted them the Federal Territory of Hawaii in 1900, set up a Territorial Board of Agriculture, for which he worked from 1902 to 1904. The Hawaiian Sugar Planters' Association then added a Division of Entomology to its Experiment Station, taking over the entomologists of the Territorial Board, with Perkins as Director of the Division. He held this post till his retirement from the islands through ill-health in 1912, after which he was retained on the staff as a consultant entomologist.

Perkins had become interested in local economic entomology largely through his friendship with the pioneer economic entomologist in the islands, Albert Koebele, a German, whose whole professional life was spent in the service of American institutions. This remarkable man, one of the first (perhaps the very first) to introduce natural enemies into any country to control insect pests, had imported the Australian ladybird beetle (*Novius cardinalis*) into California in 1888-9 to control the Cottony Cushion Scale-insect (*Icerya purchasi*), which was ravaging citrus plantations. In 1890 he sent a consignment of the *Novius* to Honolulu for the same purpose. Soon after, in 1893, Koebele, having resigned his position under the United States Department of Agriculture, took up similar work in the Hawaiian Islands, first under a Commissioner of Agriculture of the Provisional Government (before the annexation), later as entomologist to the Territorial Board of Agriculture. He left the islands in 1905 and returned to Germany, hoping to recover his health, much impaired by entomological exploration in fever-infested tropical lands. His health still failing, he was relieved from active duty in 1910, but was kept on the staff of the Sugar Planters' Association as Consulting Entomologist. After his death in 1924 three articles about the man and his work, one by Perkins and two by O. H. Swezey, appeared in *The Hawaiian Planters' Record* (29, no. 4, October 1925).

Perkins had at first been concerned only with the inspection of imported plants, a necessary procedure, as many harmful insects, accidentally introduced, had already become established. He did not meet Koebele till 1895,

as the latter's visits to Honolulu were at first few and short. But during the later years of Perkins's service under the Sandwich Islands Committee, the two men mutually helped one another. When Koebele was away in other countries searching for means to control pests, Perkins would take charge of consignments of these natural enemies on their arrival, liberating them in places favourable for their becoming established. Koebele, on the other hand, above all a field-worker with (as Perkins wrote) 'an acute perception of the habits of insects', proved a first-rate collector when in the islands, good-humouredly sharing the rigorous conditions of Perkins's later camping expeditions in the mountains.

Koebele and Perkins together were concerned with the control of a noxious plant, *Lantana*, native of the warmer parts of America. *Lantana camara*, a thorny verbenaceous shrub originally brought into the islands as a garden plant, and spread largely through the agency of certain introduced species of birds, had by the late eighteen-nineties covered wide expanses with almost impenetrable thickets. In 1897 Perkins joined Koebele in California and accompanied him to Arizona and Mexico. Koebele was hoping to find a means to control a scale-insect damaging alligator (or avocado) pear (*Persea gratissima*) in the islands, but at the same time he and Perkins investigated *Lantana* in Mexico. Afterwards many of its insect enemies were introduced into the islands, and when eventually a full account, jointly by Perkins and Swezey, appeared in 1924 (*Bulletin 16, Entomological Series, Hawaiian Sugar Planters' Association*), the suppression of *Lantana* was officially hailed as 'an outstanding example of the control of a plant pest by its insect enemies'. This may also have been the first time that insects were used to control a plant.

However, soon after its inception, the work on *Lantana* had to be temporarily put aside, while an even more serious menace was faced, namely the imported Sugar-Cane Leaf-hopper, a plant-bug less than $\frac{1}{4}$ -inch long, native of Australia. In fact, the ravages of this and of other pests of cane were the main inducement to the Sugar Planters' Association to set up their Division of Entomology. The history of the sugar industry in the islands demonstrates in high degree the risks attending the introduction of crops into new countries. The pests of these crops may also be accidentally imported, without the natural enemies controlling them in their native lands.

The search for these natural enemies involved the Hawaiian entomologists in long journeys to surrounding countries. Perkins and Koebele visited Australia for six months in 1904. Koebele, who in 1903 had visited the United States, continued his investigations in Fiji in 1905. Finally the publication 'Leaf-hoppers and their natural enemies', forming *Bulletin 1* (1905-6), published by the Sugar Planters' Division of Entomology, is a land-mark in the literature of biological control of insect pests. It comprises an Introduction by Perkins, followed by ten parts, of which he wrote seven, on the several groups of predators and parasites of the leaf-hopper.

As Director of the Division of Entomology, Perkins gathered round him a group of notable men. While working for the Territorial Board of Agri-

culture he had already engaged two assistant entomologists, the late F. W. Terry and the late G. W. Kirkaldy, the latter of whom, a specialist on Hemiptera, described the notorious Sugar-Cane Leaf-hopper as new to science under the name *Perkinsiella saccharicida*. After the amalgamation of the Territorial entomologists with the Sugar Planters' Experiment Station, two distinguished members joined the staff: the learned entomologist Dr O. H. Swezey, from the United States, about the beginning of 1905 and, at the end of that year, the late Frederick Muir who, without preliminary scientific training, had become well known from his works published while he held appointments under the Eastern Telegraph Company at Aden and at several of their African stations. How Muir took to the work for the Hawaiian Sugar Planters is proved by his having, after five years' search, many disappointments and prolonged travel in the Malay archipelago, succeeded in establishing in the islands a parasite to control the serious weevil-pest known as the Sugar-Cane Borer Beetle (*Rhabdocnemis obscura*).*

The organization built up by Perkins continues to flourish. The Hawaiian Sugar Planters' Association has maintained a staff of entomologists known not only for their work in the economic field, but for their great extension of knowledge of the endemic fauna. Their names include those of Francis X. Williams, author of a general account of *Insects and other invertebrates of Hawaiian sugar cane fields* (1931); and of Dr E. C. Zimmerman who, having relinquished economic work and having launched in 1948 the first five volumes of his comprehensive *Insects of Hawaii*, has assumed the role of successor to Perkins in studying the fauna as a whole.

Study of British insects

Perkins's more important published works on British insects are confined almost exclusively to Aculeate Hymenoptera and sawflies (Symphyta). As he wrote, 'the study of the [British] Aculeate Hymenoptera has occupied my attention for more than 60 years'. During his later years at school, having become interested in bees and wasps, he got into touch with the late Edward Saunders, F.R.S., through a nephew of the latter, a schoolfellow. Saunders, who afterwards wrote the standard work *The Hymenoptera Aculeata of the British Islands* (1896), helped Perkins by the loan of books on Hymenoptera and otherwise, and the two became constant correspondents. Perkins also corresponded with other specialists on British Hymenoptera, particularly J. B. Bridgman of Norwich and G. C. Bignell of Plymouth.

He made two collections of British Aculeate Hymenoptera. The first, largely formed before he went to the Hawaiian Islands in 1892, is rich in specimens from the districts where his father's parishes lay, especially Sopworth in North Wiltshire near the Gloucestershire border and Alderley in the Cotswolds; also those obtained near Oxford. Moreover, when engaged at

* An account of Muir's experiences is given in an Obituary by the late J. J. Walker, R.N., F.L.S. (1931, *Ent. mon. Mag.* 67, 160-162).

Cambridge on the Hawaiian material, he relaxed from sedentary work by collecting Aculeates in the bare Breck Sand district of Suffolk near Mildenhall. In 1907 he presented this collection, to which he added specimens from time to time, to Cambridge University for the Museum of Zoology.

When settled in Devon after finally leaving the Hawaiian Islands in 1912, he made a second collection of British Aculeates with, in addition, sawflies, which he presented in 1930 to Oxford University for the Hope Department of Entomology. The presentation of these two collections to University museums was emblematic of his wish to encourage younger entomologists.

Besides general accounts of *The Aculeate Hymenoptera of Devon* and *The sawflies of Devonshire*, published in 1924 and 1929 respectively by the Devonshire Association for the Advancement of Science, Perkins made a special study of the Hymenoptera and other insects of Dartmoor. Residing for many years at Newton Abbot, he collected insects on the east side of the moor, but in 1943, wishing to compare the west side, he moved to Vale Down near Lydford. Two general articles, both dealing with a small area of the moor near Lydford, appeared in *The Entomologist's Monthly Magazine* for 1943 and 1945 respectively.

His last published work consisted of short notes in the same periodical in 1945 and 1946, on immigrant Lepidoptera and the seasonal abundance of certain insects in Devon. In one of these last notes, on 'Autumnal butterflies in Devon' (*op. cit.*, 1946, p. 43), he reverted to a matter which had interested him many years before—changes in distribution among our native butterflies. Though he did not publish his earlier observations he knew that, before his time, his father and his uncle had found both the Comma (*Polygonia c-album*) and the High Brown Fritillary (*Argynnis cydippe*) abundant near Wotton-under-Edge from 1856 to 1860. Yet he failed to discover a single example of either in the district during many visits between 1877 and 1890; nevertheless, when he revisited the Cotswolds while home from Honolulu in 1907, both these butterflies had returned to their old haunts. Years afterwards he mentioned one of them, in the published note cited above, as fairly numerous in Devon; the Comma (which has spread widely in southern England during about the last 30 years).

Even his recreation resulted in an entomological article. He was from his youth a fisherman so keen that, while holding the temporary tutorship at Dartmouth in 1891, he would walk rapidly on summer Saturday afternoons 15 miles each way from Dartmouth to Buckfastleigh and back, to fish for some hours in the upper waters of the Dart. Again fishing in Devon years afterwards, his account of 'A trout's meal', in which 46 species of beetles were named by David Sharp from fragments in the alimentary canal of a single fish, was published by the Torquay Natural History Society in 1921.

In short, during the first 34 years of his 'retirement', though hampered by frequent illnesses and the consequent difficulty of visiting museums and inability to attend meetings, Perkins continued to collect and observe living insects, and produced a long series of papers and notes on British insects as

well as on material from Australia and elsewhere, including additions to the Hawaiian fauna. His versatility and energy are indicated by the titles in the bibliography.

His systematic work as a whole

Though his genius as a collector, his careful observation of the habits of insects and other animals, and his exceptional memory, have been stressed in the preceding sections, it may be well to attempt an estimate of his systematic work as a whole. What of its quality? The simplest answer is that it has stood the test of use by other specialists.

As his writings extended over 60 years (1886-1946), and most of his systematic papers appeared between 1899 and 1937, some of his methods may now seem old-fashioned. He accomplished much of his unsurpassed work on very small Hawaiian insects with only a tiny hand-lens, magnifying 15 diameters, and a small monocular microscope. He seems to have used no other instrument even when studying the minute Hymenoptera parasitic on the pests of sugar-cane, though up-to-date equipment was available at the Experiment Station in Honolulu.

'During my life' (he recorded) 'much time has been spent in the naming of insects both British and foreign for others.' For this task and for drawing up his own descriptions, he still relied largely on the hand-lens even in later years. But this method was then more usual. For instance, in the first decade of the century Dr David Sharp was also still working with a tiny hand-lens, though gradually using a binocular microscope more and more.

Perkins's extraordinary powers of sight and memory can be illustrated by two of his own expressions. He used the phrase 'a good eye for a species', which might be taken to mean a superficial manner of working; but in reality he had an amazing *flair* for swiftly recognizing the essential characters, so that he could quickly sort large numbers of minute specimens into species. He spoke, too, of the capacity 'to bear characters in one's head', that is, to remember the distinguishing characters without constant reference to books. For example, he constructed from memory, without looking at specimens, excellent keys (published in 1919) to the British solitary bees of the genus *Andrena* (nearly 70 distinct forms) and the parasitic bees of the genus *Nomada* (27 species). He explained that he had drawn up these separate keys for the two sexes from his visual memory of many specimens examined over many years, rather than from selected specimens, the characters of which may not be constant for the whole species. He added that he had afterwards verified the characters by examining specimens in every case. Nevertheless this major work narrowly escaped being rejected for publication on this score.

At the period when much of his work was done it was not so general as it now is to add a description or figure of anatomical characters, such as the male genital armature, when describing species. This was less necessary in the first descriptions of Hawaiian insects new to science, with no known close allies in the world. In any case the results could never have been published

in the time, had such details been considered indispensable. But it is fair to his memory to recall that, in the case of the British *Andrena* and *Nomada*, he did make drawings of such anatomical details for every species, though publication of these figures was unfortunately refused.

In describing species or genera new to science, he usually wrote a brief diagnosis in Latin, followed by a fuller description in English. The Latin language, so well adapted for expressing tersely the essential features, can have presented no difficulty to him; while he doubtless thought that a short preliminary diagnosis, if rightly constructed, should enable a reader trying to identify specimens to grasp the distinguishing characters of the species and at least to eliminate from consideration those which are obviously different.

The span of his life and his interests having been what they were, a Memoir of Perkins comprehends in some measure the history of the development of several aspects of entomology during that time. The great advances recently made in other branches of the subject, such as insect physiology and behaviour, have taken place largely since his active years ended. But careful systematic work remains of fundamental importance.

Conclusion

Besides the Doctorate of Science awarded by the University of Oxford in 1906, Perkins was in 1912 awarded the Gold Medal of the Linnean Society of London 'for eminent services rendered to Zoology'. He was elected into the Fellowship of the Royal Society in 1920. Having joined the Entomological Society of London (later the Royal Entomological Society) in 1903 he was, after being an ordinary Fellow for over 50 years, elected an Honorary Fellow in 1954. He was one of the founders of the Hawaiian Entomological Society, was its first President in 1905, and later an Honorary Member. He was also an Honorary Member of the Cambridge and Torquay Natural History Societies, and had been a Fellow of the Zoological Society of London since 1904.

In 1901 he married, at Waialua, in the island of Oahu, Zoë Lucy Sherrard Alatau, eldest daughter of A. T. Atkinson, some time Superintendent of Public Schools in the Hawaiian Islands (his wife's grandfather, Thomas Witlam Atkinson, F.G.S., F.R.G.S., had been a well-known explorer of Siberia, a writer, geologist, botanist and artist). Three of their four sons survive; the youngest, John Frederick Perkins, B.Sc., has been over 20 years on the staff of the Department of Entomology at the British Museum (Natural History); he is, like his father, a specialist on Hymenoptera. After the death of Perkins's first wife in 1940, he married in 1942 Mrs Clara Senior (*née* Dowse) of Newton Abbot, who died in 1949. He left Lydford (Devon) about 1952 for Lustleigh, subsequently moving in 1954 to Bovey Tracey. His last days were saddened by blindness.

I am indebted to several friends for kind help. His above-mentioned

youngest son, Mr J. F. Perkins, has given me much useful information. Mr R. B. Benson, M.A., who has been for many years the senior specialist on Hymenoptera at the British Museum (Natural History), showed me his Obituary Notice of Perkins in *The Times* of 5 October 1955, and collaborated with me in writing the notice for *The Entomologist's Monthly Magazine* of December 1955. Dr E. C. Zimmerman provided the photograph, taken by him when the subject of it was already 82 years old; and he has allowed reproduction of the two illustrations on the plate facing p. 219.

I have also drawn much on the account of his life left in manuscript by Perkins. But, apart from these sources, this Memoir is based on a friendship with him begun almost 50 years ago in the Museum of Zoology at Cambridge, and continued by personal meetings when possible, and by letters. In conversation his lively enthusiasm for entomology was infectious. Like many others younger than himself, I invariably experienced his generous help and encouragement, and was inspired by his devotion to entomological science.

HUGH SCOTT

BIBLIOGRAPHY

The titles amount to 208. The Parts of *Fauna Hawaiiensis*, each a large work complete in itself, are treated as separate items. The same applies to the Parts of the economic entomological Bulletin 'Leaf-hoppers and their natural enemies' (1905-6). On the other hand five short communications in letters, published in 1919 in *Proceedings of the Entomological Society of London*, are treated as a single item. (One title is purposely entered twice, as both ornithological and entomological, but is only counted once).

These writings vary in length from single paragraphs up to works of over 100 pages (or, in the 'Introduction' to *Fauna Hawaiiensis*, over 200 pages). Owing to the difficulty of drawing the line between the more and the less important, I have included all. In fact, many of the short notes contain information interesting in itself, or bearing on the subject matter of the longer papers. The phrases in brackets after certain titles are in some cases Perkins's own comments in his manuscript list.

He left separate lists, each in chronological order, under almost every periodical or work to which he contributed. As this involved 15 separate headings, the titles are re-arranged below in two major categories, A and B, under which are six groups in all, as follows:

A. CONTRIBUTIONS TO WORKS OTHER THAN PERIODICALS:

1. *Fauna Hawaiiensis*.
2. *Insects of Samoa*.

B. PAPERS AND NOTES IN PERIODICALS:

3. Ornithological papers.

The remainder are all entomological:

4. In *The Entomologist's Monthly Magazine*.
5. In various other periodicals.
6. Dealing with Economic Entomology.

This compromise is intended as a working arrangement. But the separation of the 108* items in *The Entomologist's Monthly Magazine* from the other entomological papers serves to indicate his long association with that journal, from the date of his earliest to that of his latest published writings (1886 to 1946). The grouping together of the papers dealing with Economic Entomology, mostly published in Honolulu, may also have some practical use.

A. CONTRIBUTIONS TO WORKS OTHER THAN PERIODICALS

1. Parts of *Fauna Hawaiiensis*

(this work contains most of the longer papers)

- 1899 (March). Hymenoptera. **1**, 1-115, pl. 1, 2.
 1899 (August). Orthoptera. **2**, 1-30, pl. 1, 2.
 1899 (September). Neuroptera. **2**, 31-89, pl. 3-5.
 1900 (February). Coleoptera, Rhynchophora, Proterhinidae, Heteromera and Cioidae.† **2**, 117-270, pl. 7-10.
 1903 (November). Vertebrata. **1**, 365-466 [Birds occupy pages 368-465].
 1910 (December). Supplement to Hymenoptera, Orthoptera, Neuroptera and Diptera. **2**, 600-700.
 1910 (December). Coleoptera Anobiidae, Bostrychidae (with D. SHARP), supplement to Cerambycidae (with D. SHARP), supplement to Curculionidae and Proterhinidae; Strepsiptera. **3**, 581-643, 645-667.
 1913 (January). Introduction, being a review of the land-fauna of Hawaiiia. **1**, xv-ccxxviii, map and pl. 5-16.

2. *Insects of Samoa* (British Museum (Natural History))

- 1928 (February). (With L. EVELYN CHEESMAN.) Part **5** (Hymenoptera), Fascicle **1**, Apoidea, Sphecoidea and Vespoidea, 1-58.
 1928 (February). Part **4** (Coleoptera), Fascicle **2**, Proterhinidae, 173-174.

B. PAPERS AND NOTES IN PERIODICALS

3. Ornithological papers (other than the account of Hawaiian birds under 'Vertebrata' in *Fauna Hawaiiensis*, **1**)

1893. Notes on collecting in Kona, Hawaii. *Ibis* (6). **5**, 101-112.
 1895. Notes on some Hawaiian birds. *Ibis* (7). **1**, 117-129.
 1901. An introduction to the study of the Drepanididae, a family of birds peculiar to the Hawaiian Islands. *Ibis* (8). **1**, 562-585.
 1906. Summary of food habits of native Hawaiian birds. *Proc. Hawaii. ent. Soc.* **1**, 26-27 [also included in list of entomological writings].
 1919. On a new genus and species of bird of the family Drepanididae from the Hawaiian Islands. *Ann. Mag. Nat. Hist.* (9). **3**, 250-252.

ENTOMOLOGICAL PAPERS AND NOTES

4. In *The Entomologist's Monthly Magazine*

1886. Aculeate Hymenoptera in 1886. **23**, 134-136.
 1887. Early appearance of *Anthophora pilipes* Fab. **23**, 249.
 1887. Notes on some habits of *Sphecodes* Latr. and *Nomada* Fab. **23**, 271-274.
 1887. Odour observable in males of *Pieris napi*. **24**, 11.
 1887. Rare Aculeate Hymenoptera in 1887. **24**, 91.

* In the Obituary Notice published in *The Entomologist's Monthly Magazine* for December 1955 (**91**, 289-291), the number of contributions to the *Magazine* was wrongly given as 115; actually Perkins's manuscript list includes 105, to which I have added three omitted by him, making 108.

† The families Curculionidae, Scolytidae and Anthribidae were treated under Rhynchophora, but Proterhinidae were then regarded as 'of uncertain position'.

1888. Notes on Aculeate Hymenoptera [group of *Andrena afzeliella* K.]. **25**, 128-131.
1889. Is *Sphecodes* parasitic? **25**, 206-208.
1890. The distribution of *Bombus smithianus* White. **26**, 111.
1890. Note on *Andrena trimmerana* Kirby and *A. rosea* Panz. **26**, 206-208.
1891. Aculeate Hymenoptera in Wiltshire in 1890. **27**, 79-80.
1891. Male and worker characters combined in the same individual of *Stenamma westwoodi*. **27**, 123-124.
1891. Aculeate Hymenoptera in S. Devon. **27**, 194-195.
1892. Stylopized bees. **28**, 1-4.
1895. On two apparently undescribed British species of Andrenidae. **31**, 39-40.
1896. A collecting trip on Haleakala, Maui, Sandwich Islands. **32**, 190-195.
1897. Notes on *Oligotoma insularis* McLach. (Embiidae) and its immature conditions. **33**, 56-58.
1899. On a special acarid chamber formed within the basal abdominal segment of bees of the genus *Koptorthosoma* (Xylocopinae). **35**, 37-39.
1899. Description of two new species of *Rhyncogonus* (Otiiorhynchini). **35**, 56-57.
1899. *Crabro planifrons* Thoms.: a species new to Britain. **35**, 110-112.
1899. *Crabro cavifrons* Thom., *C. planifrons* Thom., *C. chrysostomus* St. Farg., and *C. saundersi* m. **35**, 260-261.
1900. *Prosopis palustris* sp. nov., an addition to the British Hymenoptera. **36**, 49-50.
1900. *Odynerus tomentosus* Thoms., a species new to Britain, and some remarks on the Walcott Collection of Aculeate Hymenoptera. **36**, 172-174.
1901. Hermaphrodite Hawaiian *Odynerus*. **37**, 139-140.
1901. On a new genus of Geometridae from the Hawaiian Islands. **37**, 215-216.
1901. A new genus of Hawaiian Geometridae. **37**, 251-252.
1901. Notes on Hawaiian Aculeate Hymenoptera [including a synopsis of the species of *Odynerus* from Oahu]. **37**, 264-268.
1913. The races of *Andrena rosae*. **49**, 10-13.
1913. On an overlooked new species of *Halictus* in Britain. **49**, 62-63.
1913. *Andrena niveata* Friese probably wrongly recorded as British [this was not so]. **49**, 111-112.
1913. Notes on the British species of the *Andrena minutula* group, and correction of an error. **49**, 166-171.
1914. *Andrena nana* of British authors distinct from *A. nana* of Kirby. **50**, 34-35.
1914. Synopsis of the British forms of the *Andrena minutula* group. **50**, 71-75, 112-115.
1914. Change of name for *Andrena ambigua* Perk. **50**, 95.
1914. *Andrena mixta* Schenk: a species hitherto unrecorded from Britain [this is a white-haired variety of *Andrena varians*]. **50**, 144-145.
1914. Two hermaphroditic specimens of *Andrena*, and a monstrous form of *Salix exaltatus*. **50**, 218-219.
1914. Abnormal coupling in insects. **50**, 250-251.
1915. Two hermaphroditic specimens of *Andrena* (*A. dorsata* K. and *A. albicans* L.). **51**, 191-192.
1915. *Andrena falsifica* n.n. for *A. moricella* ♀, nec ♂. **51**, 215-216.
1916. *Andrena trimmerana* K. and its allies. **52**, 13-15.
1916. Early appearance of *Halictus morio* L., after hibernation. **52**, 67.
1916. *Nomada ruficornis* (sensu lato) and its close allies. **52**, 210-219.
1917. On a new species of *Psammochares* (or *Pompilus*) in England. **53**, 10-11.
1917. *Nomada bucephalae* n.n. for *N. lateralis* Sm. (nec Panz.) and notes on *N. conjungens* H.-Sch. **53**, 12-13.
1917. On the Kirby Collection of *Sphecodes*, *Nomada*, *Andrena* and *Cilissa*, with the description of a species of *Sphecodes* hitherto unrecorded from Britain. **53**, 45-52.
1917. Notes on the collection of British Hymenoptera Aculeata formed by F. Smith. **53**, 71-76, 159-162, 229-231.

1917. *Andrena bucephala* Steph. and *Nomada bucephalae* Perk. in Devonshire, and notes on their habits. **53**, 198-199.
1917. Note on an old specimen of *Andrena vaga* Panz. (*ovina* Kl.), a species not recorded as British. **53**, 236.
1917. New Hawaiian Caraboidea. **53**, 246-250.
1918. The synonymy of *Andrena wilkella* K. and its allies, with notes on habits, as confirming specific distinctions. **54**, 36-39.
1918. Synopsis of British Strepsiptera of the genera *Stylops* and *Halictoxenus*. **54**, 67-76, pl. 1.
1918. *Halictoxenus arnoldi*, an undescribed British Stylopidae. **54**, 107-108.
1918. Further notes on *Stylops* and stylopized bees. **54**, 115-129.
1918. The assembling and pairing of *Stylops*. **54**, 129-131.
1918. *Nomada furva* K. and its hosts. **54**, 226.
1918. On *Halictus arnoldi* E. Saunders. **54**, 253-254.
1919. A new species of Otiorrhynchine beetle of the genus *Rhyncogonus* Sharp from Laysan Island. **55**, 4.
1919. Additions to E. Saunders's Catalogue of British Hymenoptera (Aculeata), 1902, and changes in nomenclature. **55**, 8-12.
1919. Note on *Nomada roberjeotiana* Panz. **55**, 68-69.
1919. Note on a peculiarity in the burrows of *Halictus maculatus* Sm. **55**, 160-161.
1919. *Andrena dorsata* K. and *A. similis* Sm. stylopized. **55**, 181.
1920. Notes on British Psammocharidae (Pompilidae). **56**, 32-38.
1920. Note on *Epeolus* and *Colletes* (Hymenopt.). **56**, 184-185.
1921. Two new species of bees of the genus *Sphecodes*. **57**, 9-11.
1921. *Andrena jacobii* n.n. for *A. trimmerana* auct. and a new Irish variety of this species. **57**, 39-40.
1921. Monstrous form of *Salix exaltatus*. **57**, 40.
1921. Variation in British *Psithyrus* and remarks on *Bombus pomorum*. **57**, 82-87.
1922. Three hermaphrodite bees. **58**, 17-18.
1922. *Halictus tumulorum* L. and *flavipes* F. and some allied species. **58**, 24-26.
1922. The British species of *Halictus* and *Sphecodes*. **58**, 46-52, 94-101, 167-174.
1922. *Sphecodes scabricollis* Wesm. in Somerset, and description of ♀ of *S. kershawi* Perk. **58**, 89-91.
1922. Obituary notice of Vincent Robert Perkins. **58**, 110-111.
1922. Note on *Nomada hillana* Kirby. **58**, 252-254.
1924. Rediscovery of *Andrena nana* K. in Suffolk. **60**, 118-119.
1924. Female *Amauronematus* (Tenthredinidae) with partly male characters. **60**, 139.
1924. Additional notes on *Stylops*. **60**, 151-153.
1924. Dragonflies in the neighbourhood of Newton Abbot, Devonshire. **60**, 262.
1924. An *Andrena* new to Britain. **60**, 277-278.
1925. The British species of *Megachile*, with descriptions of some new varieties from Ireland, and of a species new to Britain in F. Smith's collection. **61**, 95-101.
1926. *Andrena congruens* Schm. in Wiltshire. **62**, 41.
1926. Coleoptera on burnt areas in S. Devon and a note on *Strangalia aurulenta*. **62**, 288.
1927. *Trochilium* (*Sesia*) *flaviventre* Staud. in Devon. **63**, 234.
1927. *Callimorpha quadripunctaria* in S. Devon. **63**, 234.
1927. Aculeate Hymenoptera in Devonshire. **63**, 235.
1929. Note on the black *Croesus* [sawfly] from Ireland. **65**, 18-19.
1929. Notes on British species of *Amauronematus* (Tenthredinidae), with special reference to Cameron's determinations and to the occurrence of several species not contained in our lists. **65**, 31-33.
1929. Notes on some British Aculeate Hymenoptera, including five species new to Britain. **65**, 54-58.
1929. Hymenoptera Anthophila, *Andrena*: by J. Van der Vecht [review]. **65**, 65-66.

1929. *Obrium cantharinum* in Devon. **65**, 261.
 1929. Late bees in Devon. **65**, 262-263.
 1930. The British species of *Deuteraenia* (*Agenia*). **66**, 52-55.
 1930. A revision of the British species of *Dolerus* Jurine (Hymenoptera Phytophaga). **66**, 235-248, pl. 8.
 1931. Three additions to the Aculeate Hymenoptera of Devon. **67**, 20.
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 1931. The host of *Nomada flavopicta* K. **67**, 43.
 1931. Immigrant Lepidoptera in Devon. **67**, 160.
 1933. Further notes on some British species of *Dolerus*. **69**, 19-20.
 1935. A note on some British species of *Halictus*. **71**, 104-106.
 1936. *Disenochus munroi*, a new Hawaiian beetle (Col. Caraboidea). **72**, 177.
 1936. A new species of *Proterhinus* (Col. Rhynch.) from the Marquesas Islands. **72**, 221-222.
 1937. A new Hawaiian Carabid and notes on some Nitidulidae. **73**, 125-127.
 1943. Early spring Hymenoptera and other insects on Dartmoor in 1943. **79**, 130-132.
 1945. Aculeate Hymenoptera of a small area of Dartmoor near Lydford, Devon. **81**, 145-153.
 1945. *Lampides boeticus* L. and other immigrant Lepidoptera in Devon. **81**, 216.
 1945. *Pontia daplidice* L. (Lep., Pieridae) in Devon and Cornwall. **81**, 216.
 1945. Notes on immigrant Lepidoptera, moorland insects and seasonal abundance. **81**, 251.
 1946. Autumnal butterflies in Devon. **82**, 43.
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 1946. *Sterrhia sacraria* L. (Lep.) and other immigrants near Lydford, Devon. **82**, 269.

5. Entomological writings in various other periodicals

1902. Notes on Hawaiian wasps with descriptions of new species. *Trans. ent. Soc. Lond.* 1902, 131-140.
 1902. Four new species and a new genus of parasitic Hymenoptera (Ichneumonidae, subfam. Ophioninae). *Trans. ent. Soc. Lond.* 1902, 141-143.
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 1906. Food habits of native Hawaiian birds (summary of). *Proc. Hawaii. ent. Soc.* **1**, 26-27 [also included in list of ornithological papers].
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 1906. Note on *Tomocera*, a genus of scale-bug parasites, with description of a new species. *Proc. Hawaii. ent. Soc.* **1**, 75-76.
 1907. On a species of *Proterhinus* from Samoa. *Proc. Hawaii. ent. Soc.* **1**, 87-88.
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 - (c) On *Polistes gallica* in the F. Smith Collection of British Aculeates, p. 67.
 - (d) Giantism in male bees, pp. 67-70.
 - (e) The pairing of *Stylops* and assembling of the males, observed by R. C. L. Perkins, pp. 70-75, pl. A.
1919. The British species of *Andrena* and *Nomada*. *Trans. ent. Soc. London.* 1919, 218-319, pl. 11-15.
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1920. Some new Hawaiian Coleoptera. *Proc. Hawaii. ent. Soc.* **4**, 341-359.
1921. Notes on Hawaiian Plagithmysides and Anobiides (Coleoptera), with descriptions of new species. *Proc. Hawaii. ent. Soc.* **4**, 493-506.
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1922. Some instances of hermaphroditism in bees and wasps. *J. Torquay nat. Hist. Soc.* **3**, 131-136.
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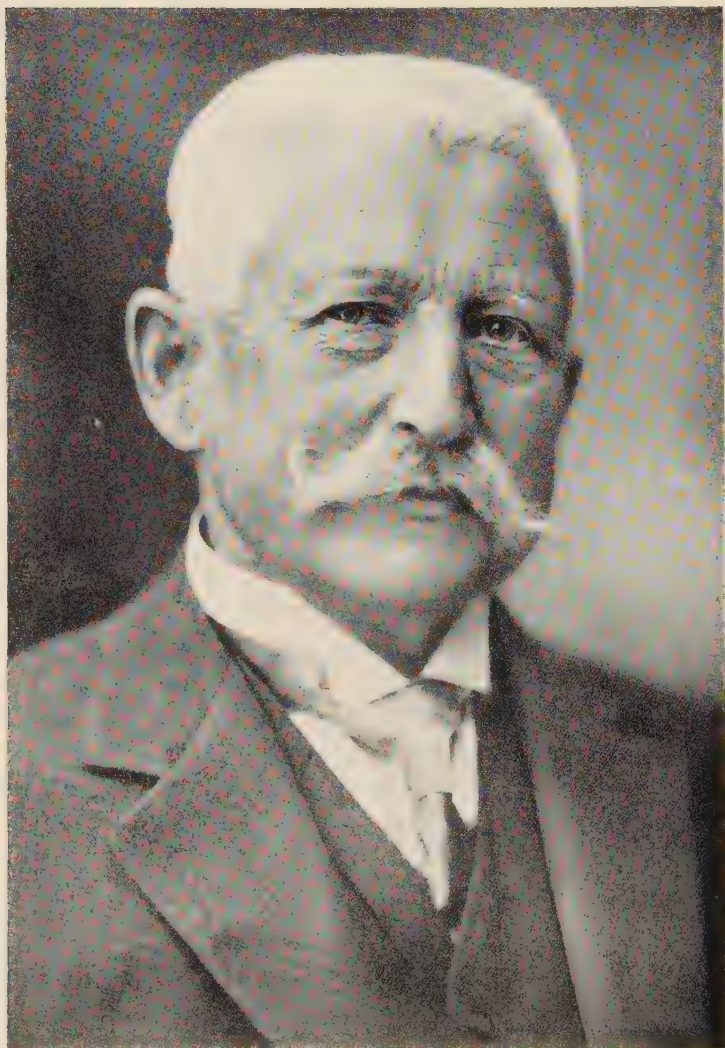
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R. Reiffer

RICHARD FRIEDRICH JOHANNES PFEIFFER

1858-1945

RICHARD PFEIFFER, one of the pioneers of bacteriology and an assistant of Robert Koch, was elected a Foreign Member of the Royal Society in 1928 at the age of 70. Twenty-seven years later inquiry revealed that he was still alive in 1945 behind the Iron Curtain, but that since then all trace of him had been lost.

It is now known that he died on 15 September 1945 aged 87 years.

Richard Pfeiffer was born on 27 March 1858 at Zduny, Posen, the eldest son of Otto Pfeiffer, a clergyman, and received his early education at Schweidnitz whither the family had removed. He passed out of the Gymnasium at the age of 17. He always had the ambition to study the natural sciences and medicine, but the family resources made a University career impossible. He was, however, fortunate in being accepted as a pupil in the exclusive 'Pépinère' (afterwards the Kaiser Wilhelm Akademie). The purpose of this institution was to train boys to enter the Army Medical Service, and a number of its pupils had become leading bacteriologists. Education at the 'Pépinère' was therefore a distinct step towards a career in medical science.

In 1879 he qualified *Unterarzt* and a year later was admitted Doctor of Medicine on a thesis '*Die anatomischen Grundlagen der Elephantiasis Arabum*'. He was fortunate while stationed at Wiesbaden to have experience of a considerable typhoid outbreak. He acquired the new techniques in the laboratory of A. Pfeiffer (no relation) and was promoted *Stabsarzt* on a thesis '*Ueber die bakteriologische Wasseruntersuchung*'.

After this introduction to bacteriology he was disgusted to find himself transferred to an 'odious' little garrison town, Dieuze in Lorraine, where he had nothing to do and was treated as a member of an army of occupation.

Only 8 weeks later his career was established. He was seconded at the age of 29 to work under Robert Koch in Berlin.

This rapid advancement was in accordance with Army policy to second promising officers to work with leading authorities. Thus in December 1887 Pfeiffer found himself in the Institute of Hygiene in Berlin University as assistant to Koch and a member of a team which included Ehrlich, Behring, Gaffky, Loeffler, Carl Fraenkel, Wassermann, Frosch, Wernicke, Kolle and others. His career in this galaxy of talent was remarkable. He not only held his own in the scientific field but by his 'firmness' and 'amiability' introduced some stability into the personal relations of his colleagues. Koch himself had

a 'lively temperament' on which Pfeiffer's calming influence was exerted without deranging a life-long mutual regard. Indeed in two years Koch made him first assistant and two years later in 1891, Director of the scientific section of the new Institut für Infektionskrankheiten. A final proof of the high regard Koch felt for Pfeiffer is the fact that he was preferred above all others as his assistant in the plague investigations in India (1896) and on malaria in Italy (1898).

On returning after his travels to the difficult atmosphere of Berlin, he had the satisfaction of being appointed to the newly established chair of Hygiene and Bacteriology at Königsberg (1899). Here he completed the construction of the research institute and organized its working. In 1909 at the age of 51 he became Professor at Breslau. Here he remained for the rest of his working life, except for short honorary appointments to Heidelberg and Berlin, and, of course, during the First War when he was Director of Hygiene in the Second Army on the Western Front ranked as 'Generalarzt'.

Although the portrait we have of Pfeiffer from contemporaries still living shows him to have been a kindly and mellow old man, it need not be supposed that he was not more forceful in his early days. He was known to have uttered 'hard words about scientific opponents' and, indeed, his writings show that he would not stand any nonsense. But he was always polite. In any case his many 'replies' to other authors should not be taken as choler, but as the normal behaviour of the day. The literature was then so small that notice was bound to be taken of contrary findings or opinions. Now, there is a reasonable chance that no one will see them.

Pfeiffer's handling of scientific opponents, difficult colleagues and matters of administration makes it clear that he must have been a man of authority, but he certainly did not use this gift to further any ambitions outside his scientific work. He is said to have been indifferent to honours and, indeed, we have record only of the Pasteur medal of the Swedish Medical Society in gold (1910) and two German awards. He, however, did appreciate the Military Order of the Crown (3rd class) bestowed on him for his work in India, which, humorously, he thought was more dangerous than active service.

His election as a Foreign Member of the Royal Society took place two years after his retirement.

He is one of the very few whose names have been enshrined in bacterial genera, but by 'some inexplicable, clerical mistake' the term 'Pfeifferella', intended by Buchanan to apply to the influenza group, became attached to glanders with which Pfeiffer had never worked (R. E. Buchanan, *General systematic bacteriology* 1925, p. 420. Baltimore. Williams and Wilkins). 'Dear old Pfeiffer, if he knew about it, would have smiled!'

Our portrait shows Pfeiffer, before his retirement, in his middle sixties.

PFEIFFER'S SCIENTIFIC WORK

Pfeiffer's major discoveries were made early in his career during a period of prodigious activity between 1892 and 1896 in which he published 34 papers. After leaving Berlin for Königsberg and Breslau his work was on a comparatively minor scale.

One of the two subjects with which Pfeiffer's name is closely associated is influenza. He was the first to describe in all essential detail and in a recognizably accurate way not only the influenza bacillus but its relationship to the epidemic influenza of 1890. In his first paper (1892) he refers to his bacillus as the 'Erreger' of influenza. There was never much doubt about his claim until in the great pandemic of 1918-19 some workers failed to find Pfeiffer's bacillus and thoughts turned to the possibility of the pandemic being due to one of the viruses which were then coming into common knowledge. As Pfeiffer complained in 1925, the refined methods of 'modern bacteriology' did not seem to him to have produced anything very convincing, and he was not alone in holding this view. However, in 1933, the influenza virus was discovered by Smith, Andrewes and Laidlaw, and it has been concluded by others that Pfeiffer's bacillus 'has lost all but historical interest' in relation to the causation of this disease. On the other hand swine influenza is accepted as being due to two agencies, a virus allied to the influenza virus and a bacillus closely related to Pfeiffer's, and many think that the pathology of epidemic influenza may more easily be explained by an association of this sort.

A leading authority in this country regards Pfeiffer's bacillus at present as 'an important and dangerous human pathogen' but whether it is a chance or essential concomitant of the virus in pandemic influenza cannot be decided until we have 'another influenza pandemic comparable in magnitude with that of 1918'.

If his bacillus has been in eclipse as an infective agent it has been remarkable as a stimulant of nutritional research. In his first paper (1892) he remarked that it could not be cultivated in series on ordinary media. In his second with Beck in the same year he argued that the slight growth in the primary culture must be due to a carry-over of nutritive material with the inoculum. He supposed that this might be 'complex proteins' from blood or sputum. Since human blood was a convenient source of complex proteins he introduced a drop onto his plates and found that subcultivation in series was then easily obtainable. In 1893 he was able to report progress. He separated blood into plasma and corpuscles and found the activity of the corpuscles even greater than of whole blood. He then lysed the corpuscles and filtered the lysate and found this filtrate equivalent to whole blood. At this point he went astray. He thought his filtrate was 'pure haemoglobin', and since this remained active after heating to 70°C and even slightly active after 100°C, it could not be functioning as an oxygen carrying mechanism. The iron must be the active principle, but he could get no action with any iron compounds available to him. Pfeiffer was quite right as to the iron pigment, proved some

25 years later to be haemin, but his 'haemoglobin' contained also a factor derived from the corpuscle, shown 45 years later to have been coenzyme I.

Pfeiffer's analysis of the growth factors of his bacillus was certainly the first effort of this sort and it remained the only effort until the outbreak of curiosity which took place during the pandemic of 1918-19. It was, in fact, the immediate cause of the development of a school of microbic nutrition and a remoter cause of a vast biochemical activity in related fields.

Following the work of Pasteur with attenuated viruses, it was clear that a process of immunization against infectious disease was possible and the study of the mechanism of this method of conferring resistance soon overshadowed the more practical aspects of the matter. It was generally agreed that infective bacteria introduced into an animal were liable to disappear and two schools of thought developed, one of which ascribed the disappearance to phagocytic activities of the body cells and the other to the killing power of the body fluids. The polemic between these two schools waxing and waning between 1890 and 1910 and longer, led to a vast amount of *ad hoc* research designed to refute the opposition. At one time it seemed that the discussion was in any case futile. In 1890 Behring showed that immunity against diphtheria and tetanus was due to neither of these anti-microbic mechanisms, but merely to neutralization of the toxins excreted by the bacteria. At that time there was a tendency to generalize on these admitted facts which clearly led to profitable commercial activities. It was, however, soon found that immunity to many or indeed most infections could not be ascribed to 'antitoxic' mechanisms and the 'humoral' and 'cellular' protagonists took new heart.

It was at this stage (1894) that Pfeiffer again came into prominence.

A great epidemic of cholera had been raging during 1892 in Hamburg and Koch was, of course, the chief scientific adviser. He had isolated the cholera vibrio and shown that the spread of the disease was by water-ways. All administrative actions were based on the idea that cholera was spread by one specific vibrio. In practice, however, it was found that many vibrios were to be detected in areas free from cholera and grave doubts developed as to whether it was wise to base action on ideas which were certainly novel rather than on the old view that cholera and typhoid depended on unknown conditions in the soil.

In this difficult situation Pfeiffer was called on by Koch to show that these intrusive vibrios were not cholera vibrios at all. Pfeiffer had already in 1889 worked on *Vibrio metchnikovi* and shown that this was quite a different organism from *V. cholerae*. His method was to inoculate guinea pigs with the two vibrios and to test those which recovered for immunity. Guinea pigs recovered from cholera were immune to this vibrio but not to *V. metchnikovi*. Similarly guinea pigs immune to *V. metchnikovi* were still susceptible to cholera. This was the first demonstration of cross-immunity tests which have had many applications to the present day. This method was applied to the Hamburg vibrios with the result that Koch's authority was not again assailed.

During this work it was noted that the vibrios inoculated into the peritoneal

cavity of normal animals rapidly multiplied but the same vibrios in an animal which had recovered from the infection underwent 'bacteriolysis', i.e. they rapidly swelled up, dissolved and disappeared in 20 minutes.

This took place without any cellular activity whatever and in fact the serum of an immune animal produced the same lysis in the test tube. This is the 'Pfeiffer phenomenon' which is still in use for differentiating closely allied species.

The chief fame of the phenomenon rests, however, on its effect on the humoral-cellular theory. As Pfeiffer remarked many years later, Metchnikoff's idea that immunity was due to phagocytosis received a check from which it never recovered. It was impossible to ascribe the lysis of vibrios in 20 minutes to phagocytosis, when manifestly there was none.

Pfeiffer had become interested in comparing cholera 'toxin' with the diphtheria toxin of his colleague Behring. Cholera seemed to him to be as 'toxic' a disease as diphtheria or tetanus, but he was unable to find any toxic substance in the filtrates of cultures until autolysis had taken place. The toxin was bound up with the vibrio and antisera made with this complex was active purely against the living vibrio, not against the toxin. He invented the expression 'endotoxin' to describe this class of substance, which he regarded as an autolytic product obtainable from non-pathogenic bacteria as well as pathogenic.

Pfeiffer, of course, attempted to use the serum of recovered animals for therapeutic purposes but found that it was ineffective owing to the rapidity of death. For preventive purposes, however, it was highly active. Pfeiffer was under some pressure by commercial interests to produce a curative serum, but declined.

It seems that Pfeiffer at one time thought that his new type of specific cholera lysin might be a valid explanation for immunity in many diseases. He and Kolle had found the same type of reaction in typhoid and Bordet's demonstration of haemolysins with similar characters to Pfeiffer's bacteriolysins, suggested the possibility of lysis being a general form of non-cellular immunity. Pfeiffer, however, was a man not easily carried away from the facts, and he readily admitted that there was something in the work of the cellular supporters. He particularly referred to the work of Almroth Wright which tended to combine the two views.

The work of Pfeiffer and Kolle on typhoid showed that animals could readily be immunized and it was not long before they made attempts to immunize man. This field had, however, been opened up independently by Almroth Wright a short time before and Pfeiffer took no part in the practical development of this routine. Curiously enough the last scientific paper Pfeiffer ever wrote, with Lubinski, in 1930, related to the futility of taking typhoid vaccine by the mouth.

A survey of Pfeiffer's work in immunity makes it clear that he was the prime originator of many fundamental concepts. He was, for instance, the discoverer of lytic antibodies in the serum of recovered animals and the first

user of such antisera for differential diagnosis of allied organisms, and indeed for the serodiagnosis of disease using known antigens. He demonstrated that these antisera had protective actions in disease and founded the scientific principles of active immunization. Thus he may well be said to have catalyzed practically all knowledge of anti-bacterial serology.

Where will be his permanent niche in a monument to advancers of medical knowledge in the practical field it is impossible to judge. It may well be argued that serum immunity is of little account, now that a chance observation, skilfully exploited in later years, has led to forms of treatment which need take no account of serological matters. However, the turning wheel will no doubt in time bring Pfeiffer's discoveries again into prominence.

His influenza work is not open to the same objection. Here he started an entirely new line of approach which was not appreciated for 25 years and then exerted an influence in very wide fields of abstract science.

Pfeiffer's work on influenza and immunity is the basis of his repute, but, of course, he touched on many other fields as a glance at the bibliography will show.

In 1892 he became interested in coccidial infections in his laboratory rabbits and was the first to describe the development cycles of this parasite. He ended his monograph with the speculation 'It is possible that the malaria parasite has a development cycle outside the human body, perhaps in lower animals (for instance insects) or in the soil. These may infect man, either through dust or water, or, as Robert Koch suggested to me, through the bite of blood sucking insects.' It will be noticed that this was 3 years before Ross described the development cycle of malaria in the mosquito stomach.

His first scientific paper described a new 'Kapsel-Bacillus', usually identified as one of the Friedländer group. In the same year, 1889, came his first paper on cholera and this subject, carrying with it the whole burden of the Pfeiffer phenomenon, occupied him for 20 years in no less than 29 papers.

According to Frosch and Kolle in Flügge's *Mikroorganismen* (1896) the description of *Micrococcus catarrhalis* there given was the work of Pfeiffer. The main features, morphological and cultural, are accurately described.

One of Pfeiffer's chief characteristics as a research worker was his meticulous and masterly technique. This may well have decided Koch's preference for his help in the field. He had also in common with Koch a great skill in photomicrography. His *Atlas* published with Carl Fraenkel as early as 1889 and subsequent illustrations to his papers are admirable.

It has been noted that after leaving Berlin Pfeiffer's output in published papers was reduced. This, of course, was due to the demands of other duties. He was responsible for the teaching of hygiene and bacteriology, and though this did not come easily to him, his lectures were of crystal clarity. His fame also attracted many assistants, and the reputation which some of these now enjoy may readily be admitted to have been earned by the care and guidance of Pfeiffer.

PFEIFFER'S END

Pfeiffer retired from the chair at Breslau in 1926 at the age of 68 and was succeeded by Carl Prausnitz, but continued to attend at the Institute for over 10 years. This was a pleasant arrangement for him because Prausnitz was an old friend. Prausnitz had been in England for some years working at the old Local Government Board's serum laboratories when Pfeiffer arrived to give the Harben Lectures in 1909. The two had been in correspondence because Prausnitz hoped to work with Pfeiffer, and this led to an invitation to translate the lectures and an offer of the hydrophobia department at Breslau. Thus from 1910 to 1923 except during the war, Prausnitz was his assistant and afterwards until 1933 in daily contact with him. The contact was ended by forces which were rapidly getting out of hand. In 1933 Prausnitz on returning from a holiday in England was arrested. He was not detained long, but, being half English, a mounting disgust decided him and he left Germany.

The record of Pfeiffer at this time shows him to have been a man not only respected as an ornament of former days, but liked by all members of the community. He was an institution available to all for advice and help. He gradually gave up work at the bench but his mental processes remained adequate and he attended the local scientific meetings. He seldom spoke but when he did it was final.

He still acted as co-editor of the *Zentralblatt für Bakteriologie*.

Physically Pfeiffer at this time was robust. He spent every Sunday in the mountains accompanied by unsuspecting new arrivals at the Institute who were made to act as pace makers.

In more sedentary fields Pfeiffer was an accomplished pianist and exponent of Chopin. He amused himself also with his own compositions. His interest in music caused him to adopt a niece of his wife and have her trained as an operatic singer. He lost his wife in 1934.

After Prausnitz left, Pfeiffer carried on at Breslau but in a world which was changing and becoming distasteful. In 1938 on his 80th birthday he was made the central figure of a Nazi rally in the University and a year later he withdrew to Bad Landeck where he had bought a little house. Bad Landeck (now called Łądek-Zdrój) is a picturesque little watering place in the mountains separating Silesia from Czechoslovakia. He had often been there on holidays and had friends there. In fact he took a new wife there, his wife's niece already mentioned. The death of his second wife a year after he settled at Bad Landeck was a great shock to him, but he was sustained by his devoted maid Frau Hedwig (Hedel) Köhler, his two doctors, one Dr Reisky an old pupil, and a retired Generalarzt Dr Fischer, his wife and daughter.

He became a well known and respected figure in the town on his daily pilgrimage with 'Hedel' to the Forest Cemetery. He played Chopin to his friends and prided himself on his garden. He was still receiving proofs for the *Zentralblatt* and was a voracious reader, always taking 3 books simultaneously.

On his 86th birthday he impressed a circle of friends with an hour's discourse on his scientific life.

This was in 1944. The rumblings of war hardly penetrated to Bad Landeck. But suddenly came disaster. The Russians overran Breslau and the flight to the West began. Pfeiffer was left behind or, it is thought, declined to leave. Bad Landeck was occupied on 8 May 1945. The old man was treated with consideration. A Russian major, billeted on him, became friendly and visited him later. However, the fighting troops soon moved on and the occupation was taken over by the satellite Poles. His house was confiscated and he was confined to one room on the first floor, out of reach of his piano and even of his bed. 'Hedel', however, could not be deterred and looked after him.

And so he drifted on until the day when Erna Fischer came to break to her 'Onkel Richard' the news of her father's death. This seems to have been the breaking point. Next day, 15 September, when 'Hedel' brought him some supper upstairs, she found him lying on his sofa in physical distress. Someone was playing his piano below. She hurried for his doctor friends. One was ill in hospital, one in prison and a third found him dead.

He was buried in the churchyard of Bad Landeck Parish Church on 19 September 1945, in the presence of Frau Köhler and his remaining friends.

The facts, on which this account is based, are derived almost entirely from the writings and inquiries of Pfeiffer's old friend and successor at Breslau Dr Carl Prausnitz Giles, of Ventnor, Isle of Wight. He has read and approved the account.

Professor Dr J. Kathe of Rostock, who knew Pfeiffer at Breslau and occasionally heard of him up to the time of the Russian occupation, has been good enough to supply photographs and a 'preview' of an obituary notice to appear in the *Zentralblatt für Bakteriologie*.

The last picture of her old Geheimrat is, of course, based on letters from Frau Hedwig Köhler.

PAUL FILDES

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A. O. Rankine

ALEXANDER OLIVER RANKINE

1881-1956

ALEXANDER OLIVER RANKINE was born at Guildford in 1881, the son of the Rev. John Rankine, a Baptist minister. Both his father and mother were of Scottish descent. He had one brother and two sisters. He was educated at the Royal Grammar School, Guildford, after a period at a Board School. From Guildford he went with a county major scholarship to University College London, where he graduated with first class honours in physics in 1904. In 1910 he became a D.Sc. of the University. He was assistant in the Department of Physics in University College from 1904 to 1919, except for a period of war service from 1917-1918. He worked then under W. H. Bragg (later Sir William) first at Aberdour on the Firth of Forth, then at Harwich as Deputy Resident Director under Professor (then Colonel) A. S. Eve, and finally as Director of a new station at Dartmouth, or rather Kingswear. This involved some administrative responsibilities, and he showed tact and ability in handling the problems in war-time of an experimental station concerned with getting scientific gear into service. His main personal research contribution was a device, called the Photophone, for transmitting speech by means of a beam of light, but he also worked on acoustic and electromagnetic means of submarine detection. For his work in the War he received the O.B.E.

Soon after the end of the War, in 1919, he was appointed Professor of Physics at Imperial College, South Kensington, in succession to Lord Rayleigh, this was the second chair in the department, Callendar holding the first. However, when Professor F. J. Cheshire retired in 1925 from the post of Director of the Technical Optics Department, Rankine was appointed to that post, which he held until 1931 when the Technical Optics Department was amalgamated with that of Physics, and he returned to his former position. In the 1930's he became interested in the instruments used in geophysical prospecting, and became head of the section of geophysics, then part of the Physics Department, where he directed the teaching and research. In 1937 he left Imperial College to become Chief Physicist of the Anglo-Iranian Oil Company. He had been an Adviser of this company since 1927, and continued his association with it as Geophysical Adviser till 1954, though he retired from being Chief Physicist in 1947. In this connexion he paid a number of visits to Persia, which he enjoyed, though the conditions were often rather primitive for a man of his age. His work for the Company was mainly advisory, he does not seem to have done any important personal research, but those who had to do with him speak warmly of his helpful advice and encouragement in

technical problems. During the War period 1942-45 he was seconded for special work with the Petroleum Warfare Department, and also did work for the Admiralty and Ministry of Aircraft Production.

Rankine played an important part in scientific societies. Thus he was a founding Fellow of the Institute of Physics, and was its Honorary Secretary and Vice-President (1923-37 and again in 1943-47). He was President of the Optical Society at the time of its union with the Physical Society in 1932, a union which he did much to bring about, and President of the Physical Society 1932-34. He had before been Librarian (1920-24), Business Secretary (1924-29) and Vice-President (1929-32) of that body. He was President of Section 'A' of the British Association in 1932, after having been Recorder. Later on he helped to organize the very successful Optical Group of the Physical Society, of which he was Chairman in 1942-45. He was elected a Fellow of the Royal Society in 1934.

Rankine was an excellent lecturer, both in the ordinary routine of university life and on special occasions. He gave the Christmas lectures at the Royal Institution in 1932-33, his title being 'The round of the waters'. After one of his lectures he was addressed by an old man who told him that he had attended these lectures ever since he was nine, and had sat in the same seat in the gallery. On the first occasion when Tindall was lecturing, so the old man said, an elderly gentleman sat next to him. Questions were asked for, and the gentleman nudged him and said, 'Here, you, get up and ask so and so', which in some timidity he did. Tindall paid no attention, but the old gentleman urged him to repeat his question, and then at last Tindall looked up and said, 'Ah, it's you, is it, I thought so'. The old gentleman was Faraday!

He gave no less than five Friday Evening Discourses, three on geophysical problems and two on fog dispersal. His connexion with the Institution was a long one, and he had been a regular attendant at the lectures, and served as Manager and Visitor before he became Secretary (1945-52). His tenure of the office of Secretary was clouded by a series of disputes in which he and the Resident Professor appeared as protagonists. These ended in the virtual dismissal of the latter, and a law suit in which Rankine had to give evidence. Very strong feelings were roused in the scientific world, and this is not the place to attempt to allot praise or blame, beyond saying that Rankine felt a compulsion to try to prevent by any possible means what he conceived to be an injustice. Soon after this his health began to fail. He died on 19 January 1956.

Rankine's first research, as far as I have been able to find, was work which he did under Professor Trouton at Univeristy College on 'Elastic after-effects' (Trouton & Rankine, *Phil. Mag.* **8**, 538, 1904). The method involved was to keep the strain constant and measure the change of stress with time. How far this was Rankine's idea is uncertain, but the experimental methods were ingenious and the results of some importance at the time. This was followed two years later by a paper of his own on 'Decay of torsional stress in solutions of gelatine'. However, he does not seem to have followed this much further,

and his next interest, also with Trouton, was on the problem of ether drift. With Troughton he made accurate measurements of the possible change in resistance of an electric wire as it was turned, and so presumably altered its relation to the hypothetical ether drift. A null result was established to a high order of accuracy, and it was shown that this null result could be explained by the Fitzgerald-Lorentz contraction and the electronic theory of metallic conduction.

Many years later Rankine returned to a similar problem, this time in conjunction with Silberstein. He examined the propagation of a plain polarized beam of light in the earth's gravitational field, and showed that no rotation occurred or, more precisely, that the fractional change in the velocity of light polarized vertically and horizontally was less than 8×10^{-13} . The extreme sensitivity of the method makes it an interesting test of gravitational theory.

But the work for which Rankine is best known is that on the viscosity of gases. In 1910 he published in the *Proceedings of the Royal Society* an account of an extremely simple and ingenious method of measuring the viscosity of gases of which only small amounts were available. It was, in fact, designed to measure the viscosity of neon, xenon, and krypton, specimens of which he was able to get on loan from Sir William Ramsay. The method depended on the rate of fall of a pellet of mercury in a vertical tube, the two ends of which were joined by capillary tubing to form a closed circuit. The pellet could be made to move backwards and forwards between fixed marks by inverting the tube and the time of fall was measured. The method was suitably modified for gases which would attack mercury. For the viscosity of volatile vapours he evolved a method also involving flow through capillaries in which the driving pressure was supplied by the vapour pressure of the liquid. With a slight modification this was also used for the vapour of iodine. The original apparatus as modified and improved was capable of comparing the viscosities of two gases with an accuracy of about one part in a thousand, and needed only a few millilitres of the gas. The absolute accuracy was about 0.5 per cent. The accuracy was improved by a suggestion of Kuenen and Visser (*Comm. Phys. Lab. Leiden*, No. 138, 1913 (3)), according to which the mercury pellet could be broken into two parts, and measurements taken with the same pellets single and double, thus eliminating a good deal of the difficulties due to surface tension. The apparatus was suitable for temperatures from that of the room to 100°C and Rankine used it over this range. The interest in this work lay not merely in the measurement of viscosity at different temperatures for gases, some of which had not been tested before, but in the conclusions drawn as to the shapes and sizes of the molecules and the character of the forces between them. The viscosity of a gas gives a measure of the free path of its molecules and so of the area of the molecules effective for collision. This effective area is found to vary with temperature, and Sutherland had shown that the variation could be accounted for by supposing an attractive force between the molecules. Measures of the viscosity over a range

of temperature enable a constant to be determined, known as Sutherland's constant, and from this and the viscosity at any one temperature one can find the true collision area, assuming the theory to be valid, and the molecules spherical. Hence, in this last case, one can find the molecular radius. By 1920 the analysis of crystals had proceeded sufficiently far for W. L. Bragg to estimate the sizes of the atoms in solids, and Rankine was able to compare these with those deduced from viscosity. The latter, in fact, turned out to be substantially larger, even in the case of the inert gases the sizes of which were deduced by Bragg from the consideration of the space occupied by the atoms of the corresponding halogens. Rankine considered that the reason was that thermal collisions never lead to so close an approach as that which occurs in crystals.

Rankine next proceeded to consider the behaviour of molecules of more than one atom, for which it was obviously unreasonable to assume a spherical shape. He was able to show that a good agreement with the experiments could be obtained by constructing what he called the 'mean collision area' of the molecule from the known sizes of the atoms, allowing for overlap so as to get the distance between atomic centres found by Bragg. A projection of this area averaged over all directions was substituted for the corresponding area of a spherical atom in the kinetic theory formula for the viscosity.

Though this method is an approximate one (it would not, for example, work for very long thin molecules) it gave satisfactory results for a number of molecules to which Rankine applied it. These included CO_2 , COS , and SO_2 and a number of hydrides, and led to correct conclusions as the relative positions of the atoms in the molecule. The arguments based on viscosity have been long superseded by other more powerful methods of determining the arrangements and spacing of atoms in gaseous molecules, but Rankine's work was important as showing that these arrangements were accessible to experiment, and the work formed an additional link between physics and chemistry which stimulated thought on these lines.

During World War I while Rankine was at Harwich he started work at the suggestion of Sir William Bragg on a method of transmitting sound by means of a beam of light. The method involved the use of selenium cells. By an ingenious device, the movement of a small mirror, caused by a diaphragm which could be moved either directly by the sound pressure or indirectly through a telephone, was made to change the position of the image of a fine grid so that it varied in phase with another real grid of opaque and transparent elements, sometimes blocking the light and sometimes not. In this way the amount of light sent through could be controlled with great sensitivity. Using the sun as a source, transmission was possible up to a distance of several miles, and the reproduction of speech was good. The selenium cell, is, however, notoriously tricky, and though Rankine did a good deal of work on it after the War, and was able to show that one of the common forms of trouble, a special kind of polarization, could be removed by drying out the cell, the method was never widely used. He adapted it later to the talking

film, but it does not appear to have come into commercial use, at least in the form he gave it.

Rankine's interest in applied geophysics went back at least to 1928 when he gave a paper on seismic methods to the Institution of Mining and Metallurgy, and in 1929 he gave Royal Institution lecture on 'Physics in relation to oil-finding'. Not long after this he took charge of the Geophysical section of the Physics Department at Imperial College, and began practical work with an Eötvös gravity meter of a type modified by Dr H. Shaw and Mr Lancaster-Jones. He was able to cure this instrument of a serious defect due to electrification. He was led from the study of the gravitational balance to attempt a magnetic analogue, and though he was unable, for what seemed to be fundamental reasons, to produce an instrument for use in prospecting he did, in fact, produce one extremely sensitive to small changes in the local magnetic field. It was used for the measurement of the susceptibilities in weak fields of para- and dia-magnetic materials.

During World War II Rankine took charge of an attempt to find a method of fog clearance on airfields. The problem, like all those involving convection, is a difficult one, but Rankine was able to arrange a model in a wind tunnel in the Empress Hall at Earl's Court, and to show that the measurements so obtained were valid for the full scale. The methods which were devised by him and his co-workers were, in fact, used under the name of FIDO, for fog dispersal during the latter part of the War. It was effective, but very expensive, consuming 100 000 gallons of petrol per hour.

Rankine's experimental work was marked by economy of effort. His apparatus was the simplest that would do the job, and it did it very well. His viscometer, his photophone and his magnetometer are all models of instrument design. Though his magnetometer is now superseded it did excellent work in its day, and some of his measurements of viscosity are still quoted after more than thirty years. While not a theoretical physicist in the modern sense of the term he could use mathematics effectively, and made deductions of theoretical importance from his experimental results.

His work on committees was important. He was a very good committee man, partly because he was generally liked, but perhaps even more because of his powers of clear thought and clear expression. He lived through the age when physics was much less organized than it is now, and in less need of organization since it was mostly confined to the universities. He did a good deal to ensure that the organization developed on sound lines.

Rankine had a strict upbringing, which neither drove him into reaction nor prevented his natural humanity and good humour from being fully developed. He had not many hobbies, except a little gardening and a pleasure in music, mostly as a listener. He was for many years a supporter of the Baptist church at West Ealing, but later moved to Hampton. In 1907 he married Ruby Irene Short of Reading, by whom he had two sons and two daughters. When he was in charge at Dartmouth during World War I the family moved down and lived in the upper part of the Royal Dart Hotel, the

lower part of which was used for the experimental work. He and Mrs Rankine entertained his scientific and naval helpers at musical parties on Saturday evenings. Even much later he was both capable and willing of singing comic songs, and he and Sir Richard Paget sometimes performed together, especially at meetings of the Physical Society dining club. He was a fluent and amusing speaker, both on formal and informal occasions. As a colleague he was a rock of strength, reliable and charming, he always did what needed doing with the minimum of fuss. He believed in leaving research students alone until they really needed help, then he gave it willingly and efficiently. He was a most unselfish man, and I have good cause to be grateful for the way Rankine took my place, and the efficiency with which he did my work at Imperial College, during a long illness.

He is survived by his wife, by his two sons and by two daughters, one married and one now a widow.

G. P. THOMSON

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A. Rosenheim

SIGMUND OTTO ROSENHEIM

1871-1955

OTTO ROSENHEIM was born at Würzburg in Germany on 29 November in the year 1871. He married Mary Christine Tebb, a daughter of William Tebb of Rede Hall, Burstow, in July 1910, and died in his Hampstead home on 7 May 1955, his wife dying in 1953 in her 85th year.

Rosenheim chose the University in his home town of Würzburg in which to study for his degree of Doctor of Philosophy and he worked under Tafel in Emil Fischer's laboratory. Part of the course he spent in Bonn and then returned to Würzburg to complete his degree. The title of his thesis was 'On the oxidation of *p*-hydroxyquinoline'. His examiner was Hantzsch who had succeeded Fischer in the chair of chemistry and Rosenheim was the first student to be examined by Hantzsch. The oral examination was a difficult one, but Rosenheim came through with flying colours.

A year's military service in the horse artillery then followed, Rosenheim being excluded from taking officer rank. He then went to Geneva to work with Graebe and there met Liebermann, Pictet and Kehrman. He was called up for a month's military training in Germany but asked to be excused as he was finishing his course. He had already made up his mind to leave Germany and go to England, where a cousin had already settled, as anti-semitism was abhorrent to him. He wrote to W. H. Perkin at Manchester and told him of his desire and Perkin invited him to come to work at the University. Rosenheim left Switzerland for Germany to settle his affairs and he is recorded as having entered for research in chemistry in Manchester University for the session 1894-1895.

In 1896 Rosenheim joined Philip Schidrowitz in practice as analytical and consulting chemists at a laboratory in Chancery Lane in London. Dr Schidrowitz writes of Rosenheim at that time as a modest and pleasant young man much interested in his work especially in that of a scientific character; he was a remarkable craftsman, excelling in glass-blowing, photography and in manipulative procedures. His main interest was, however, in biological chemistry.

This last sentence aptly summarizes Rosenheim's outlook and career; he was a born biochemist and a deeply read student and master of the subject throughout his life.

In 1901 Rosenheim was appointed Research Student of Pharmacological Chemistry in Tunncliffe's Department of Materia Medica and Pharmacology at King's College in the Strand. He had already collaborated with

Tunncliffe before the latter's appointment to the Chair on an investigation involving the volumetric estimation of uric acid and on the use of piperidine as a uric acid solvent. Once in Tunncliffe's department a steady flow of original work under their joint authorship followed on such subjects as the influence of borates and of formaldehyde on the metabolism of children, the influence of selenium on certain tests for arsenic and on selenium compounds as factors in a beer-poisoning epidemic. Whilst in this latter field Rosenheim noticed the decomposition of compounds of selenium and tellurium by moulds. The whole of the experimental work in these investigations was carried out by Rosenheim.

In October 1904 Rosenheim was appointed a Lecturer in Chemical Physiology in Halliburton's department on the recommendation of the Natural Science Board of the College. Here he was fortunate in meeting F. S. Locke with whom he collaborated in their classical investigation on the effect of certain sugars on the isolated mammalian heart and on the quantitative disappearance of dextrose when perfused through such a heart. They devised an ingenious new perfusion method for the mammalian heart using dextrose in oxygenated Ringer's solution; they followed the consumption of sugar and found that if calcium or potassium were omitted the amount of sugar consumed fell off. The amount of carbon dioxide formed ran parallel with the consumption of sugar.

On Locke's departure Rosenheim turned to the detection of choline in circulating body fluids, a subject on which Halliburton and Mott had already made some observations. Rosenheim used the polarizing microscope to aid him in his diagnoses and this instrument was always on his bench, at his elbow, throughout his career. It was but a short step from choline as a breakdown product of nervous tissue to the chemistry of the brain which was Rosenheim's chief problem for many years.

In 1906 he devised a simple method for the isolation of cholesterol from brain by mixing the minced brain material with plaster of paris, and sand, and extracting the cholesterol with acetone at room temperature, thus avoiding the difficulties presented by the phosphatides which are insoluble in cold acetone. In the following year Rosenheim, in collaboration with Mary Christine Tebb, a Bathurst Student of Girton College who was gaining research experience in Halliburton's department, laid low the bogey of protagon as a definite chemical compound. As early as 1865 Liebreich had isolated a crystalline material from brain by extraction with solvents and had called it protagon. Thudichum in 1874 in his great work on brain chemistry advanced evidence in favour of protagon being a mixture, but Gamgee and Blankenhorn and later Cramer obstinately believed it to be a single chemical entity. Rosenheim and Miss Tebb showed how, by simple extraction with different solvents, protagon could be resolved into two crystalline substances, phrenosin and kerasin free from phosphorus and a residue of sphingomyelin containing phosphorus. Polemics followed but the findings of Rosenheim and Miss Tebb have stood the test of time. Whilst examining the optical activity

of their products these workers found that protagon dissolved in pyridine showed a slight positive rotation which changed to a high laevo-rotation due to the separation of sphaero-crystals of sphingomyelin. This phenomenon they called sphaerorotation.

Rosenheim was a meticulous worker and in this difficult field he spent a considerable time in working out methods for obtaining pure phrenosin and pure kersasin, his introduction of the selenite plate test greatly facilitating the separation. He also showed that these substances occur free and uncombined in brain. A series of communications on the galactosides of the brain culminated in 1916 in a paper in which constitutions were advanced for phrenosin and kersasin which are close to those now accepted.

Rosenheim had pre-eminently an enquiring turn of mind and he was always fascinated by colour reactions. He was intrigued by Hopkins and Cole's statement that the Adamkiewicz reaction for tryptophan is due to glyoxylic acid in the acetic acid used. This was the sort of test-tube problem Rosenheim revelled in and according to him the impurity in the glacial acetic acid was probably formaldehyde, impurities in the sulphuric acid used also playing a part and with these reagents he was able to reproduce the Adamkiewicz reaction and the same spectrum.

The subject of pressor bases, a popular subject in the first decade of this century also attracted Rosenheim's attention. Dixon and Tayler thought that the placenta had a normal secretion of a pressor substance but Rosenheim showed conclusively in 1909 that if there was no putrefaction there was no pressor effect. One product of putrefaction Rosenheim identified as tyramine.

In 1915 Rosenheim had the title of Reader in Biochemistry conferred on him, but he resigned his Readership in 1920 to devote all his time to research, the Delegacy recording their pleasure that he would continue to carry on his research work in the Department of Physiology in the College.

After a few years of quasi-retirement in his Hampstead Garden City home during which Rosenheim devoted much time to his rock-garden, with annual excursions to the Alps and Mediterranean islands on plant hunting expeditions, Rosenheim and his wife came in late 1923 to the laboratories of the Medical Research Council in Hampstead with the problem of spermine. They had already worked out exact conditions for the reproduction of the somewhat mysterious spermine phosphate crystals, first observed by Leeuwenhoek in 1678, whilst Rosenheim was at King's College, and stimulated by the discovery of insulin in 1923 by Banting and Best, Rosenheim thought, wrongly as it proved, that there might be some connexion between spermine and insulin, especially as there was no known function for spermine.

This was the beginning of a second productive period in Rosenheim's life. With Dudley and later with Starling's preparative assistance he isolated spermine in quantity from various tissues, in particular pancreas, discovered a new base spermidine, determined their constitutions and finally synthesized both bases.

Rosenheim cogitated continuously on the biochemical problems of the time; in particular the food accessory factors interested him absorbingly. With Kajiura in 1908 and 1910 he had paid some attention to rice and the beri-beri problem, but the fat-soluble accessory food factors presented a wider field of study and experiment. With Drummond in 1920 he discussed the possibility of a relationship between the lipochrome pigments and the fat-soluble accessory food factors and in 1925 they showed that the well-known colour reaction of cod liver oil with sulphuric acid could be replaced to advantage by one using arsenic trichloride. The colour produced had a greater permanence, was amenable to measurement and gave an approximate assay of the content of vitamin A. This marked a great step forward.

A fruitful period of collaboration then followed with T. A. Webster who was in Leonard Hill's department in the Institute and who had considerable experience in dietetic experiments with rats. Rosenheim and Webster showed in 1927, from colorimetric and biological tests, that the amount of vitamin-A present in liver fats other than that of the cod often exceeded that present in the latter. Thus the liver oils of fish such as salmon and halibut are sometimes 100 times as rich as cod liver oil. This was a discovery of industrial importance. Naturally in a field like this, Rosenheim's flair for colour reactions received full play and many were the publications revealing insight and ingenuity centering around the exploration of this theme.

Webster had also worked out a reliable technique for testing antirachitic materials on rats, so Rosenheim and he collaborated in their assault on the problem of the nature of vitamin-D. In 1925 Drummond, Rosenheim and Coward had found that the precursor of the active substance found in food-stuffs on exposure to ultra-violet radiation was in the lipids and was associated with the sterols, cholesterol and phytosterol. Starting from this point Rosenheim and Webster found that dihydrocholesterol and dihydrophytosterol were not activatable and if they started from cholesterol, irradiated it and removed unchanged cholesterol by digitonin precipitation, the filtrate yielded an antirachitic preparation active in daily doses of 0.01 mg for rats. In discussions with Rosenheim at this time I had suggested to him that the rather unstable activatable material might be ergosterol, known to occur in plant sources—incidentally I had isolated ergosterol earlier on from *Amanita muscaria*—and he reminded me of my surmise when he and Webster in 1926 studied many sterols and their derivatives and found that ergosterol and its acetate were almost the sole substances which could be activated. Further work by Rosenheim and Webster in this country and by Windaus and Hess abroad established the fact beyond doubt that pro-vitamin-D was indeed ergosterol. The larger-scale work founded on Rosenheim and Webster's observation was undertaken in the Institute by Callow, Bourdillon, Webster and their collaborators and resulted in the isolation and characterization of vitamin-D or calciferol in 1931.

From his early work on cholesterol of the brain, Rosenheim took a keen interest in the degradative studies of Windaus on cholesterol and of Wieland

on the bile acids and often sought my opinion on the latest structures proposed. We were agreed that the blocked quaternary carbon atom looked wrong. When the paper by Diels and Gädke appeared in 1927 on the production of chrysene from cholesterol by hot palladized charcoal I drew Rosenheim's attention to it with the comment that I was sure it would revolutionize views on the structure of cholesterol. To our surprise as the months passed by, the observation of Diels seemed to have been ignored or considered as a deep seated pyrogenic change of little constitutional significance. Rosenheim however thought that no one in Germany dared question the authority of Wieland and Windaus. Meanwhile ergosterol had come to the fore and the numerous facile movements of double bonds in this substance as revealed by the work of Windaus and Heilbron convinced us that the old sterol-bile acid formulae with the blocked quaternary carbon atom were impossible. To be convinced that a structure is definitely wrong is half the battle in the development of any new formula.

Early in 1932 Bernal had reported his X-ray examination of sterol crystals as being difficult to reconcile with the accepted structures. Rosenheim was clearly perturbed and kept harping on it and sought my opinion. Following on this I happened to re-read Ingold's discussion of Diels's work in the Chemical Society's *Annual Reports* for 1927 and was unconvinced that the side-chain of cholesterol took part in the formation of chrysene. When however Bernal's second note appeared in *Nature* in 1932 and Rosenheim came to see me anxiously and asked for my opinion, an idea which must have come to me subconsciously flashed across my mind and I said to Rosenheim 'Why don't you take ring-II in the Windaus-Wieland structures and put it on the other side of ring-I so as to produce a potential chrysene structure?' Next morning Rosenheim came to me and exclaimed excitedly 'It fits, it fits'. My comment was that the new arrangement was bound to fit some of the facts of degradation; when we went into it more deeply and found that Windaus' degradation of ring-I which puzzled him by leading to a four-membered ring ketone, on our formulation led to a five-membered ring we felt we were on the right lines. After further study of the problem we arrived at the structure of cholesterol and the ring-system of the bile-acids now accepted universally. At the time we realized that our structures would revolutionize the chemistry of the saponins and heart poisons, the sex-hormones and the principles of the adrenals, for all would have to take on the same pattern but we never envisaged the accelerating flood of original communications based fundamentally on our formulae which have appeared in the last 23 years.

It was not often that Rosenheim made incursions into the realm of pure organic chemistry but from 1933 to 1943 he and his collaborators Callow, Starling, Stiller and Petrow studied the action of selenium dioxide on sterols and problems connected with the diols and triols derived from cholesterol. For years Rosenheim had been puzzled by the resinous product called 'oxycholesterol' by Lifschütz, obtained from cholesterol dibromide by

debromination with sodium acetate and which gave a blue colour with various sterol reagents. By the action of selenium dioxide on cholesterol Rosenheim and Starling isolated *cis*- $\Delta^{5:6}$ -cholestene-3:4-diol and by the action on cholesterol esters a *trans*- $\Delta^{5:6}$ -cholestene-3:4-diol. Both of these substances, the first of their type, readily gave cholestenone on treatment with dilute acids and blue colours with sterol reagents. The *trans*-diol was very sparingly soluble in organic solvents and this enabled the authors to show that the *trans*-diol was a constituent of Lifschütz's 'oxycholesterol'.

At the other extreme Rosenheim was at home in metabolic studies. He often spoke of the puzzle of coprosterol formation which was known to occur on feeding brain-cholesterol to dogs and in 1943 with Webster he took up this problem experimentally in feeding experiments on rats. They established a number of striking facts. In the first place they found that feeding cholestenone in diets poor in cholesterol to rats resulted in a large increase in the excretion of faecal coprosterol. Secondly, they identified cholestenone in the faeces of dog and rat after feeding brain substance. Thirdly, they established that a thermostable dietary factor was concerned in the conversion of cholesterol into coprosterol and finally that coprosterol formation was inhibited by the simultaneous presence in the diet of succinylsulphathiazole or carbarsone. In a similar manner β -sitosterol is hydrogenated and excreted as coprositosterol.

Rosenheim often expressed his admiration for the work of Thudichum. One day I drew his attention to the name Thudichum in the London Telephone Directory. He was thrilled, in an undemonstrative way by this and soon arranged a meeting with the Thudichum family and was able to secure for science many of Thudichum's historic bulk specimens with their elaborate descriptive labels and his note-books. Among Thudichum's specimens was glycoleucine, an amino-acid he isolated from brain and which was generally regarded as norleucine. A collaborative effort in 1945 between Consden, Gordon, Martin, Rosenheim and Synge revealed that Thudichum's glycoleucine was in fact *dl*-leucine.

With such a record of positive achievement it is satisfactory to record that Rosenheim was elected into Fellowship of the Royal Society in 1927.

In the laboratory Rosenheim was neat and methodical; he was thorough; he was meticulous, sometimes over so, but these traits were great assets when he handled the large number of substances he accumulated from many sources for all kinds of colour and other tests. The specimens were packed in neatly labelled boxes and classified so that no time was lost in finding specimens in answer to many requests for samples. He had an unusual flair for mixing solvents for recrystallization purposes and when he had finished with his recrystallizations there was never a doubt of the purity of his specimens. He was greatly attracted by gadgets; with some of these he adorned his working bench, many being gadgets to light his pipe, for he was an inveterate smoker of a straight pipe. He always liked to help the young research worker; in fact I first met him on the stairs going up to a Biochemical

Society Meeting at one of the London Hospitals and he stopped me and enquired after some research work of biochemical interest of which I had given an account at an earlier meeting. This was typical of the man. No one ever consulted him without profit for he was sagacious and had a phenomenal memory for all things biochemical. He was ever ready with suggestions and if any one expressed interest in any phase of his work he would be delighted to give an account of it, one of his mannerisms being an occasional furtive glance at the listener to see if he was paying attention. He was of a placid, contemplative disposition, had no great sense of humour and had sound original views on many subjects of public interest. He loved to spend some of his time in libraries, getting to the bottom of things as he called it, tracing observations and statements right back to their origin. In speech he never quite lost an accent which betrayed his German origin.

Rosenheim's chief hobby was his rock-garden, but he was also a keen and artistic photographer, his studies of still life in particular earning the commendation of the connoisseur.

For many years Rosenheim served as a much-valued member on the Accessory Food Factors Committee of the Medical Research Council. He was also a Fellow of the Linnean Society.

Rosenheim left no personal record for the Royal Society and was averse to family matters being mentioned in connexion with his scientific work. He nursed his wife practically unaided for the last two years of her life and this adversely affected his own robust constitution, but he never complained, was always philosophic and survived his wife by a few months short of three years.

H. KING

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J. Winton

JOHN ALEXANDER SINTON

1884-1956

FEW can have had so varied experiences in life and at the same time contributed so greatly to knowledge in a field of science as Sinton. Educated and taking his medical qualifications in Northern Ireland, and first in every examination he took in school or university, entering the Indian Medical Service, again the first on his batch, Sinton was to see active service in both world wars. In the first, as medical officer to an Indian cavalry regiment on active service in the Mesopotamian Expeditionary Force, he received the award of the Victoria Cross for conspicuous bravery and devotion to duty during an action at Sheik Sa'ad in 1916, thus having the unique distinction of being the only bearer of this honour to be also a Fellow of the Royal Society. Later in the Medical Research Department of the Indian Medical Service he was appointed the first Director of the Malaria Survey of India (now the Malaria Institute of India) an institution that under Sinton's direction and that of those following him was to become one of the chief centres of malaria research in the world. Then after retirement, but being still on the reserve of officers of the Service, he was in the second world war again recalled to military service and as Consultant Malariologist in several campaigns again saw active service in many countries, travelling extensively in India, the Middle East and Africa and later, as Adviser on Malaria to the War Office, again to travel and advise on measures against malaria, visiting Australia, New Guinea, the Solomon Islands and other areas. Finally retiring to his native Ulster at the age of 60 he took an active part in academic and public affairs and besides other activities was Pro-Chancellor of his University and in 1953 High Sheriff of Co. Tyrone.

It was to research, however, that Sinton's inclinations impelled him and throughout his career he never ceased to make contributions to medical science, publishing some 200 scientific papers, most of them dealing with malaria, but including a series of some 40 papers on the systematics and structure of sandflies, a subject that seemed to have a fascination for Sinton from the time of his early studies when he worked with Professor Newstead at Liverpool on these flies, later shown to be vectors of oriental sore and kala azar. Only a few days before his death Sinton learnt that he had been awarded the Manson Medal, the highest honour given by the Royal Society of Tropical Medicine and Hygiene awarded triennially to the living author of such original work in tropical medicine or tropical hygiene as the Society thought worthy of the honour.

Parentage and early life

John Alexander Sinton was born on 2 December 1884 at Victoria, British Columbia, the third of seven children of Walker Lyon Sinton and Isabella Mary Sinton (*née* Pringle), the sequence of births being a son, a daughter, a son and four daughters.

His father's people, the Sintons, were of original lowland Scottish origin, settled in Co. Armagh, North Ireland, for more than 250 years, during which time most of them belonged to the Society of Friends. They were mainly engaged in the linen industry and agriculture, but many emigrated to the United States. A cousin of Sinton's grandfather married Charles Taft, a brother of President Taft. Sinton's paternal grandmother (Hemmington) came from Chatteris, Cambridgeshire, and also belonged to the Society of Friends.

The Pringles were also of lowland Scottish origin settled in Co. Tyrone, Monaghan, for about 250 years. They were mainly of professional and agricultural occupation. Two of Sinton's uncles on his mother's side were doctors and his mother's cousins included: (1) the late Professor H. Pringle of Trinity College, Dublin; (2) James Pringle, K.C., formerly M.P. for Tyrone and Fermanagh; (3) Robert Pringle, barrister, Dublin; (4) Seton Pringle, formerly President of the Royal College of Surgeons of Ireland; (5) J. C. Parke, solicitor and Davis Cup tennis player, International rugby player and International golf player; (6) several solicitors. Sinton's maternal grandmother (Mackie) came from Nairn, Scotland, of a family mainly of professional and agricultural occupation. Both the Pringles and the Mackies were Presbyterians. Sinton's brother, the eldest of the family, was Director of James Mackie and Sons, Belfast, manufacturers of textile machinery, when he died aged 38.

The family came to Ulster in 1890, when Sinton would be about six years old. They were never in affluent circumstances and it was to his mother's influence, her hard work and unsparing devotion to educate and bring up her children in their proper sphere of life that Sinton ascribed any success her children may have had. She was widely read and a great lover of poetry.

Schooling and medical education

Sinton's primary schooling was at the Nicholson Memorial School, Lisburn, Co. Antrim, where he was from the age of nine to fifteen. His secondary schooling was at the Royal Belfast Academical Institution (1899-1902) from which he matriculated in the Royal College of Ireland and entered the Art's School of Queen's College, Belfast. A year later he passed the First Arts Examination of the Royal University of Ireland and entered the Medical School of Queen's College. Here success followed success in prizes and honours in almost every subject or examination he undertook. He was prizeman in botany, physics, zoology and chemistry, with a special prize in the

scholarship examination and took First Class Honours in Botany and Physics with 2nd Class Exhibition at the First Professional Examination. In his second year he was prizeman in a number of subjects, passing his Second Professional examination with Upper Pass and recommendation to proceed to honours. In his third year he was again prizeman in anatomy, physiology, materia medica and other subjects, passing his Third Professional with Upper Pass and recommendation to proceed to honours. In 1907 as fifth year scholar he was prizeman in surgery and midwifery and Gold Medallist in children's diseases. When graduating M.B., B.Ch., B.A.O. of the Royal University of Ireland, he took First Class Honours in Medicine, Second Class Honours (first place) with Exhibition in Surgery and First Class Honours with Exhibition in Midwifery and Gynaecology. After graduation he was House Surgeon and House Physician at the Royal Victoria Hospital, Belfast, and afterwards Riddell Demonstrator in Pathology at Queen's University. At this time he was also Hon. Pathologist to the Benn Ulster Eye, Throat and Ear Hospital and Clinical Pathologist to the Mater Infirmorum Hospital, Belfast.

Whilst at Queen's University Sinton took the D.P.H., Belfast (1910) (first place and special prize) and the D.P.H. Cambridge (1910). After a course in tropical medicine at Liverpool he took the D.T.M., Liverpool (1911), again taking first place. From February to August 1912, Sinton was Research Scholar, under a post-graduate exhibition of Queen's University, at the Liverpool School of Tropical Medicine, where he became acquainted with Sir Ronald Ross and Professor J. W. W. Stephens. In August 1911 Sinton joined the Indian Medical Service (taking first place in the entrance examination), but before proceeding to India was seconded for a year as research exhibitor under a post-graduate exhibition of Queen's University, during which he studied protozoology at the Liverpool School of Tropical Medicine.

The Indian Medical Service

The Indian Medical Service offered many attractions to those young medicals just passed through the schools and desiring an active life abroad, since, besides suitable salary and pension, there was a wide field of choice in different Government appointments, such as civil surgeoncies, specialist appointments with private practice, professorships in colleges or institutions, research appointments and other openings. The Service was, however, primarily a military one and on joining, an officer, until seconded to 'Civil', served in some military capacity, very commonly as Medical Officer to an Indian Regiment. Usually this period of military service lasted for a few years only, unless as some preferred to do they elected to remain in 'Military'. It was the experience of most young officers to spend these few years under peace conditions, living more or less the usual life of the European in India. This, however, was not to be Sinton's experience, for before the expiry of his period

of military service there occurred the first world war so that he had many years of active service and it was not until the end of the war, actually 1921, that Sinton left 'Military' for 'Civil'.

Military Service

Sinton's first appointment was as regimental medical officer to the 31st Duke of Connaught's Own Lancers stationed at Kohat in the North West Frontier Province. He was also in charge of the Brigade Laboratory and at this time published several papers. One recorded the results of a study of Roger's method of treatment of cholera by hypertonic saline as seen in the treatment of 69 cases under his care. He also, in collaboration with his assistant, contributed a paper on eosinophilia in helminthiasis, giving a careful record of results in 150 cases including infections with *Anchylostoma duodenale*, *Ascaris lumbricoides*, *Trichuris trichiura* and *Oxyuris vermicularia*. On the outbreak of the Great War 1914-18 and after being R.M.O. and S.M.O. Moveable Column, Kurram Valley, in the N.W. Frontier, with the rank of Captain, he was posted to the Indian Expeditionary Force D (Mesopotamia) as R.M.O. 37th Dogras and other units. It was in an engagement at Sheik Sa'ad at the time of the attempt to relieve Kut in 1916 that Sinton gained the great honour and distinction of being awarded the Victoria Cross.

The citation records that although shot through both arms and through the side he refused to go to hospital and remained as long as daylight lasted, attending to his duties under very heavy fire. The citation also notes that in three previous actions he had displayed great bravery. In the same campaign he was mentioned in dispatches on four occasions and received, from the Russians, the Russian Order of St George.

On return to duty Sinton, still in 'Military', was on active service in a number of theatres of war until 1920. He was for a time Sanitary Officer with the rank of acting Major in the Mahsud Operations on the N.W. Frontier, later in the East African F Force (Tanganyika) and then with the East Persian Cordon Field Force commanding a Cavalry Field Ambulance and Agency Surgeon, Khorassan. From August 1918 to April 1919 he was in the Turkestan Military Mission as S.M.O. and commanding a Cavalry Field Ambulance with rank of Lieut.-Colonel. In 1919 for a year he was D.A.D.M.S. (San.) successively in the Afghanistan Campaign 1919, the Mahsud Campaign 1919-20 and the Waziristan Campaign 1920. Many times Sinton was mentioned in dispatches and received many awards and medals. He was also awarded the O.B.E.

Mention has been made that the Indian Medical Service was primarily a military service and even though an officer had been seconded to 'Civil' he was still liable to recall in case of war. On the outbreak of the second world war Sinton, being still on the reserve of Officers, though then retired, was recalled to duty, but in April 1940, having reached the age limit was placed on the retired list and returned home, joining the Home Guard as a

private. From June to November 1940 he was, however, re-engaged by the Royal Army Medical Corps as Consultant Malariologist to the East African Force and from April 1941 to April 1943 was employed by the Government as Consulting Malariologist to the Middle East Force, travelling in this capacity in many countries in the Middle East as well as in 1942 being on a tour to report on health conditions on the projected air routes for troops across Africa, visiting Soudan, French Equatorial Africa, Nigeria, the Gold Coast, the French Cameroons and the Belgian Congo. From March to July 1945 Sinton was on an inspecting and advisory tour for the Army to see malaria conditions in Assam, Burmah and Ceylon and on to Australia, New Guinea, the Solomon Islands and the Moluccas. When he finally retired at the age of 60 Sinton had served over 30 years, of which more than a third was spent on active service.

To return, however, to the termination of the first world war and Sinton's subsequent life in India whilst in civil employ. After a period of leave home, during which he worked under Professor Newstead studying sandflies of the genus *Phlebotomus* and under Professor J. G. Thomson on protozoological studies, Sinton returned in 1921 to India and was taken on the cadre of the Medical Research Department. For some years he was in charge of the Quinine and Malaria Enquiry under the Indian Research Fund Association and then in 1927 appointed the first Director of the Malaria Survey of India, in which appointment he remained until his retirement in 1936.

At this stage it seems desirable to explain briefly the nature of the appointments referred to and to give a brief description of the organization for research upon and control of malaria in India during Sinton's time, an organization in the growth of which Sinton played an outstanding part. This can best be done by giving a short account of the Malaria Survey of India and the steps that led to its coming into being.

The Malaria Survey of India

The Medical Research Department of the Indian Medical Service was constituted under the administration of Col. J. T. W. Leslie I.M.S., Public Health Commissioner with the Government of India in 1904. It consisted of a sanctioned cadre of officers filling appointments in the Central and Provincial Government Laboratories and other Government sanctioned permanent posts. As time went on, increasing demand for additional researches and enquiries resulted in special grants for these being made by the Government. This eventually became the function of the Indian Research Fund Association, a body established in 1910 which received a special grant from the Government but was largely independent, in this much resembling (and ante-dating) the Medical Research Council in Great Britain. This Association financed such enquiries and other activities as were approved and sanctioned by its Governing Body. A further important development was the institution some years later of the Indian Medical Research Workers

Conference, which was held annually at Calcutta, Delhi or other such location as might be decided upon and was attended by delegates from all parts of India. At such meetings, as a result of discussion, proposals for researches or enquiries were made that were usually accepted by the Indian Research Fund Governing Body and implemented. At the Meeting of 1925 sanction was obtained, as a result of proposals put forward, for a malaria organization for India to be known as the Malaria Survey of India, the functions of which were briefly to be as follows:

- (1) To be fully informed on all malaria problems. To advise the Government on all issues relative to malaria in India.
- (2) To initiate enquiries and investigations on malaria.
- (3) To undertake systematic research in due course into basic facts underlying malaria transmission, prevalence and prevention.
- (4) To carry out epidemiological surveys and mapping.
- (5) To advise upon and assist in carrying out antimalarial measures; to study these scientifically and to judge of and elucidate the results.
- (6) To undertake clinical work on malaria including treatment.
- (7) To assist affiliated researches.
- (8) To teach and train officers and others in practical malaria work.
- (9) To publish scientific, results, useful guides and bulletins.

Such an organization did not start entirely *de novo*. As early as 1916 an organization for India had been put forward and implemented following the Imperial Malaria Conference held at Simla in that year. Under this there was formed a Central Malaria Committee and a Provincial Malaria Committee for each of the eight Provinces of India. Under the Central Committee there was established a Central Malaria Bureau at Kasauli where collections were started, a reference library begun and a malaria journal, *Paludism*, instituted. Under the Central Committee were also held classes of instruction in malaria field work and survey. Under the Provincial organizations malaria units under the direction of senior malaria officers were set up. As a result of this organization for the first time knowledge was gained regarding the local conditions relating to malaria throughout the greater part of India. The Malaria Map of India (1926) published under the names of the present writer and Sinton was almost wholly based on such work. The recall of almost all officers connected with the organization during the first world war led, however, to the virtual closing down of all such activities.

At the time of Sinton's return to India following the war the Central Malaria Bureau had been re-established at Kasauli and there were a certain number of enquiries and researches in progress. One of these was the Quinine and Malaria Enquiry financed by the Indian Research Fund under Sinton. There was also an important enquiry known as the Malaria in Sind Enquiry in which the conditions present in a new large irrigation project were being closely watched with a view to such measures being undertaken as would

prevent the usual increased malarial incidence that followed upon such large-scale irrigation. There were other investigations in progress about this time, including one into the reason for a high malaria incidence at Karnal and measures to control this.

Under Sinton's direction these various activities were brought together under the new organization. The headquarters of the organization was fixed at Kasauli, incorporating the Central Malaria Bureau and its activities. A permanent field station and laboratory was established on the plains within easy access from Kasauli at Karnal, where a building, a large unoccupied hospital belonging to a Mission, was acquired, admirably adapted for the purpose with museum, lecture hall and ample accommodation for laboratory work. Here and at Saharanpur the malaria field classes were re-started and held annually. Besides temporary workers engaged in special enquiries a considerable staff was on the permanent cadre including an entomologist, a chemist and an engineer, with the necessary laboratory assistants, collectors and other personnel.

In December 1937 at the Research Workers' Meeting at Delhi the Government of India decided to take over the Survey as regards its public health and advisory functions and the whole organization, now named the Malaria Institute of India, was transferred to its present site at Delhi, by now the Imperial Capital of India. This, however, was after Sinton's time. But the paper 'Man-made malaria' made a deep impression on the Conference and undoubtedly had a considerable effect in the decision of Government to afford to the organization the status it now holds.

As Director of the Malaria Survey of India Sinton was *ex officio* a member of the Scientific Advisory Board of the Indian Research Fund Association under which the scheme was then financed and later a member of the Governing Body. He was also a member of the Malaria Commission of the League of Nations and Co-ordinating Officer for India in this body. In 1929 the Malaria Survey issued the first number of its journal, *The Records of the Malaria Survey of India*, now the *Indian Journal of Malariology*. During Sinton's time as Director there were published in the *Records* 195 papers, including 61 giving the results of malaria surveys or local enquiries into malaria, 75 relating to systematic and bionomical studies on *Anopheles*, including control measures, and 46 on treatment, parasitology or other studies on malaria. Other papers were published in the *Indian Journal of Medical Research* or in other journals. During the same period there were published two volumes of the *Fauna of British India* dealing respectively with the anopheline and culicine mosquitoes of India.

Life and work in India

Sinton published some 200 scientific papers, some in collaboration with others, but mainly in his own name. For the greater part these deal with various aspects of malaria: clinical studies; studies of the malaria parasites;

researches on treatment and the evaluation of the new antimalarial drugs; immunology of malaria, especially as studied experimentally in monkey malaria; laboratory techniques; and other studies in relation to this disease. A certain number of these communications, however, relate to other subjects among which Sinton's contributions to the systematics and structure of sandflies of the genus *Phlebotomus* are particularly outstanding.

Some of this large volume of work was carried out in India, some when on leave or after retirement at home. Much of Sinton's earlier work on treatment was done at Kasauli where a Treatment Centre had been established at the Cantonment Hospital for the treatment of cases of malaria among British troops from all parts of India.

Kasauli is a hill station on the first ridge of the Himalaya foothills at which is situated the Government of India Central Research Institute and the Pasteur Institute of India for the treatment and study of rabies. It was a station from which there was easy access to Simla, then the summer headquarters of the Government, as also to Delhi, the winter headquarters, and indeed to the Punjab and India generally. It was here that Sinton for many years had his headquarters and where much of his work was done and where he had many facilities and opportunities of collaboration with his colleagues working at the above laboratories and hospital.

It was at Kasauli that Sinton met and married on 19 September 1923 Eadith Seymour Steuart Martin, daughter of Edwin Steuart Martin and Ada May Martin (*née* Martin), and where their daughter Eleanor Isabel Mary Sinton was born on 9 December 1924.

Later as Director of the Malaria Survey Sinton spent much time in the Field Station laboratories at Karnal and in visiting other places in India. When first appointed Director he made a three months' tour, visiting all centres where records of work on malaria in India were likely to have been kept in preparation of his 'Bibliography of malaria in India', published in the first number of the *Records*. It was as a result of this three months' tour that Sinton re-published in the following number of the *Records* the classical paper by Dempster (1847) on an enquiry into malaria in a canal area north of Delhi in which for the first time use was made of the spleen rate in mapping and measuring the intensity of malaria.

Many of Sinton's later observations, especially those on *Plasmodium ovale* and the newer antimalarial compounds were, however, made in England working as Manson Fellow of the London School of Hygiene and Tropical Medicine and at the Ministry of Health Malaria Laboratory at Horton, where unique conditions for such researches existed.

Researches on the malaria parasite

Sinton's work was notable for the many contributions he made to different techniques connected with the study of the malaria parasite, improved methods of preparing and using thick films and interpreting appearances seen

in these and especially in the enumeration of parasites and their identification. He was one of the first to repeat Bass's method of culturing the parasite and his technique for the fowl-blood method of enumerating parasites has been widely employed in studying the course of infections and in determining the parasite values in endemic malaria. In collaboration with Ghosh (1934) Sinton made important observations on malarial pigment, investigating the action of solvents and of oxidizing and reducing agents upon its optical properties and crystallization. He was especially interested in the changes produced by different parasites on the red cell and only a few months before his death he contributed a paper to the Jubilee number of the *Indian Journal of Malariology* discussing points that were still unsettled in our knowledge of the malaria parasite. In collaboration with Hutton and Shute he published a series of papers on *Plasmodium ovale* making a very thorough study of this parasite. His paper in collaboration with the present writer (1928) on the correct name of the malignant tertian parasite by pointing out the confusion that existed in the nomenclature was largely responsible for the taking up of this subject by Mr Francis Hemming, Secretary of the International Commission on Zoological Nomenclature, and the subsequent establishment of the names now in common use through an official opinion by that body, thus ending the confusion that had resulted from unsatisfactory attempts to use names which different authors thought to be correct.

Researches on malarial treatment

In his earlier work on treatment of malaria quinine was still almost the only drug available for this purpose and Sinton added much to the knowledge of how it should best be used. In particular he pointed out the value of using this drug in combination with alkali with a view to increasing the alkalinity of the blood. He determined by controlled experiment the effect of different dosage and duration of treatment upon the relapse rate and gave many practical points to help those responsible for its use, including a simple method of testing the amount of quinine in medicinal solutions. As other potentially effective compounds became available Sinton early undertook controlled research on their role in treatment and was the first to point out the importance of plasmoquine (since called pamaquin) in reducing the relapse rate, a feature which in this and other drugs was later to become a very important field of research. In collaboration with Hutton and Shute Sinton added greatly to knowledge of the newer anti-malarial compounds in the treatment of infections with the different parasites and in particularly in the search for drugs bringing about true causal prophylaxis, i.e., so acting that infection was prevented even when the parasite is inoculated by bite or otherwise. Among drugs so tested were stovarsol, pericrina 303, sodium stovarsol, quinine troposan, parosan dimeplasmine, atebtrin, plasmoquine, certuna, proseptasine, and paludrine. There were many issues involved in such work. There was the question of effectiveness against the different

parasites. There was the question of cure, i.e., complete elimination of infection, as against treatment of the attack, which however effective did not necessarily eliminate infection and so prevent relapses, one of the chief objectives in malaria treatment and one which quinine, though very effective against the attack, largely fails to do. There was also the question of the effectiveness of different drugs in prophylaxis and the degree to which a drug might be capable of true causal prophylaxis. In all these cases there was the question of dosage in treatment and in prophylaxis and many other issues to be determined. As a result of Sinton's and his collaborators' researches much valuable information regarding the treatment of malaria was obtained.

Researches on the malaria parasites of monkeys

During 1932-35 Sinton, largely in collaboration with Harbhagwan and with Mulligan, carried out intensive researches on the plasmodial infections of monkeys, being among the first to undertake such studies and through them to be able to utilize experimental methods in the investigation of treatment and in other directions. In a series of papers the authors gave detailed descriptions of the parasites and their identification together with a critical review of the literature. By these studies they were able to learn much about the mechanism of immunity in malaria, the implications of multiple infections and super-infections and to estimate the effect of treatment upon the development of immunity. They were also able to demonstrate an intradermal reaction in such infections. As a result of this and later work Sinton published in the *Journal of the Institute of India* (1939) a paper entitled 'A summary of our present knowledge of the mechanism of immunity in malaria'.

Researches on the systematics and structure of sandflies of the genus Phlebotomus

Sinton's work on sandflies of the genus *Phlebotomus* began with his association with Professor Newstead at Liverpool in collaboration with whom he published in 1921 a paper entitled 'On a collection of Papataci flies (*Phlebotomus*) from India'. In 1923 Sinton published the first of a series of 36 papers dealing with Indian species of this genus the last of the series being in 1933. He also published descriptions of some species from other countries than India. In all some 27 species were described (24 from India and Ceylon) including 16 that were new to science. These descriptions were very thorough, running as a rule from 5 to 8 pages of print and dealing, not only with external characters, but with internal structure. It was in this last respect that Sinton's work was more especially notable and some of the important characters used in identification and recognition of species were structural features such as the pharyngeal armature, and in the female, characters of the spermathecae. Without this intensive study and the use of such characters identification and affinities of species would scarcely have been possible.

It was incidentally Sinton's work on the pharyngeal armature in *Phle-*

botomus which led to the description by Sinton and Covell of the buccal cavity and pharyngeal armature in *Anopheles*, and by Sinton and Barraud of these structures in the Culicini. Especially in *Anopheles* the pharyngeal armature has been a particularly important character in identification and classification.

Other researches

Sinton has made other contributions to science than those so far mentioned. He was one of the first (1917) to describe and give the stages of development of the parasitic trematode *Agamodistomum* in *Anopheles*, previously recorded only from *A. maculipennis* in Europe and now described by Sinton in the larva and adult of three species of this genus on the North West Frontier of India. He also early described other helminthic infections in this genus. Sinton in collaboration with Little (1925) was also one of the earliest to note the occurrence of the midge *Culicoides* as an ectoparasite of *Anopheles*.

Besides describing oriental sore in Russian Turkestan, Sinton, in collaboration with Shortt, was the first to record cutaneous leishmaniasis in a dog in India, canine leishmaniasis having been previously described only from the Mediterranean area. Later Sinton, in collaboration with Shortt and Swaminath (1935) obtained transmission of leishmaniasis to humans by the sandfly *P. argentipes* fed on oriental sore in a monkey and also successfully obtained transmission of cutaneous leishmaniasis by inoculation of humans from a natural lesion in a dog in India.

Though the study of endemicity in malaria has not been a special feature of Sinton's work he has nevertheless contributed papers dealing with surveys carried out in India and his recommendations to the Malaria Committee of the League of Nations on malaria conditions in the delta of the Ebro in Spain is an important contribution to knowledge of European malaria. Sinton was also joint author with the present writer of the 'Malaria map of India', published in the *Indian Journal of Medical Research*, giving the distribution of endemic and epidemic malaria in contours of intensity, a map that is almost, if not quite, unique for any tropical country.

Nor would mention of Sinton's contributions to malariology be complete without referring to the many helpful bulletins issued from the Malaria Bureau and the infusion of keenness and interest which Sinton aroused among those with whom he was associated. Here too should be mentioned the contributions that Sinton made in some of his papers regarding economic aspects of malaria. 'What malaria costs India' is deservedly famous and a remarkable summary of the economic effects of malaria, which the writer of an obituary has noted might appropriately be called 'What malaria costs the world' so extensive is its scope and documentation (Covell, *Brit. Med. J.*, 7 April 1956). To this might be added another remarkable paper entitled 'Man-made malaria', which Sinton read at the 13th All-India Congress of Medical Research Workers held at Calcutta in 1935 and which has probably never been equalled as a description of certain epidemiological features of

malaria not often described or their importance realized. Another contribution has been 'Malaria in war', the Robert Campbell Memorial Oration read at the Ulster Medical Society in 1946. Sinton's appreciation in *Current Science* (1946) of the work of Sir Ronald Ross should also be mentioned, as also Sinton's bibliography of 2200 titles of papers and reports on malaria in India since Ross's discovery of the mosquito cycle, a work including many unpublished records of useful work which alone is an outstanding contribution to malariology.

During 1944-45, when in England before retiring, Sinton made many contributions that were published as helpful reports or circulars by the Army, as also by the Ministry of Health and by the Ministry of Transport giving information and instructions regarding precautions against malaria and its treatment. Some of these were for merchant seamen and ships' doctors. Some were translated for use in France. As in all Sinton's work and in whatever task he undertook there is evidenced in these reports and circulars a meticulous care and thoroughness that is very characteristic.

Life in retirement and public honours

Sinton in 1945 retired with the courtesy title of Brigadier, to private life on his estate at Cookstown, Co. Tyrone, Northern Ireland, with the intention to take up fishing and ornithology, pursuits for which he had always had a great liking. During his life in India Sinton must have had many opportunities when time and leave permitted to enjoy these hobbies, if so they might be called, to watch the eagles at Kasauli circling the crags and perhaps to fish in some swiftly flowing Himalayan stream or to enjoy one or other of the many happy relaxations that India offers.

But soon Sinton was to enter wholeheartedly into local affairs. Interested in his old University, he became a member of the Senate in 1948, was Pro-Chancellor in 1952 and President of Queen's University Association 1953-54. He was Governor of the Royal Academical Institute, Belfast, 1946-50; President of the Co. Tyrone Branch of the Forces Help Society (formerly the Sailors', Soldiers' and Airmen's Help Society) 1946-52, and Vice-President of the North Ireland Branch of this Society from 1952. He was President of the Cookstown Branch of the British Legion 1946-53, member of the Council of the North Ireland Branch 1949-52, and represented North Ireland at the 11th Conference of the British Empire Service League at London in 1951. In 1946 he was made Hon. Member of the Royal Society of St George. He was President of the Queen's University Service Club from 1946; Vice-President 1946-52 and President 1952-3 of the Old Instonians Association. He was Magistrate (J.P.) for Co. Tyrone from 1947; High Sheriff for that County in 1953; and Deputy Lieutenant on 1954.

In the course of his service and in retirement Sinton received many honours and distinctions. Besides the Victoria Cross, the many Mentions in Despatches and Medals for war service in the many campaigns in which he

took part and the award of O.B.E. during his military service, he received many civil awards and distinctions. He received the Hon. Degree of M.D. of the Queen's University in 1917 and in the same year the Arnott Memorial Medal of the Irish Schools and Graduates Association. He was made Hon. Member of the Ulster Medical Society in 1920. He was awarded the Chalmers Medal of the Royal Society of Tropical Medicine and Hygiene in 1929 and was made Hon. Member of the National Malaria Society, U.S.A., in 1930. From 1943-46 he was a Member of the Malaria Commission of the Medical Research Council and in 1944 was awarded the Bissett-Hawkins Medal of the Royal College of Physicians. He was a Vice-President of the Royal Society of Tropical Medicine and Hygiene 1945-47 and President of the Malaria Commission of the League of Nations 1937-46. In 1946 he was elected a Fellow of the Royal Society. In the same year he was Robert Campbell Orator and Medallist of the Ulster Medical Society. In 1949 Sinton received the Mary Kingsley Medal of the Liverpool School of Tropical Medicine. From the Egyptian Government he was awarded the Anti-Gambia Memorial Medal in appreciation of his services in the malaria epidemic of 1950. In 1953 he was made Hon. Fellow of the newly formed National Society of India for Malaria and Other Mosquito-borne Diseases.

As regards Sinton's personal qualities one has the impression of an acute and active mind, almost tireless in its many activities. He was greatly liked by his colleagues and socially he had a charm that with his natural kindness, generosity and reliability gained him many friends and admirers. He had an extremely high sense of duty and once he had made up his mind on a course of action he believed to be right he was not easily deviated from it, a characteristic that at times gave him a little of the proverbial Irishman's love of a fight. This did not prevent him, however, being popular with his colleagues and with those in authority. He was a great lover of children and animals and was essentially kind and gentle, a characteristic noted by the writer of one of his obituary notices and one regarding which the present writer is in agreement.

Sinton died on Sunday night, 25 March 1956 at his house in Cookstown, Co. Tyrone, at the age of 71, and was buried with full military honours among the mountains near his home. He leaves a widow and a married daughter.

RICKARD CHRISTOPHERS

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W. A. Spencer

WILLIAM KINGDON SPENCER

1878-1955

I. Biographical notes

WILLIAM KINGDON SPENCER, who died at Ipswich on 1 October 1955 in his 77th year, achieved world-wide recognition among palaeontologists as an authority on Palaeozoic starfishes; but to a larger section of the community he was known as an outstanding figure in the world of education. He was born on 10 December 1878 at Barrow on Soar; but the greater part of his boyhood was spent in the Calder Valley, where his father, John Firth Spencer, was head master of the village school at Blyth. He attended his father's school between the ages of 3 and 12 years; and his contact at that impressionable period with the home weavers of the district gave him a keen sense of craftsmanship.

In 1890 he gained a West Riding scholarship to Batley Grammar School. Here, under the direction of an enlightened head master, he learned to work for himself rather than to be 'crammed'; and he always considered himself fortunate to have come under the influence of W. Rushby who, as science master, 'gave the inspiration which could lead to an understanding of the joy of discovery and of the emotional satisfaction accompanying it'.

At Batley he was a contemporary of Sir Owen Richardson, and in 1897 both of them obtained scholarships to Oxford. Spencer's scholarship was not available to him until October, 1898, and he spent the intervening year at Owen's College, Manchester. There he attended classes in geology given by Professor Boyd Dawkins which stimulated his already considerable interest in that branch of science. His scholarship to Oxford had been granted in chemistry, but his tutor at Magdalen suggested that he would be better advised to read for honours in zoology and geology; acting on this advice he graduated with first class honours in 1902.

After graduation he spent half a year at the University of Marburg under Professor E. Korschelt, primarily to familiarize himself with German literature and scientific method. Here he studied the central nervous system of the Phyllopod Crustacea and published an account of his researches. He was convinced that his early research was largely responsible for his election to a Burdett Coutts post-graduate scholarship on his return to Oxford; but he characteristically deplored the 'many howlers' that he had perpetrated.

For a time Spencer worked under Professor Sollas at Oxford, acting as lecturer and demonstrator as well as carrying out research on the Cystidea.

Although this work was not taken very far, it undoubtedly had the effect of concentrating his interest on Palaeozoic echinoderms, and so led to his outstanding contribution to our knowledge of the early starfishes. In 1904 he married Miss Kate Violet Gordon Stewart and took up a position as Lecturer in Biology at the University College at Bangor; but being unable to wait for an adequately remunerative academic appointment, he joined the Board of Education as an Inspector of Schools. Thus a potential professional in science became an amateur. During the thirty-four years of his service it was only in his spare time, as a relaxation from his official duties, that he carried on his palaeontological research. His work as an officer of the Board of Education is appraised in the second section of this notice.

During the early years of his Inspectorship, while working in Whitehall, Spencer lived in succession in Cambridge, Dulwich and South Croydon. On his appointment as Inspector for East Suffolk he finally settled in Ipswich in 1914. He retired from the Board of Education in 1938. Then, in view of his wife's failing health, and partly to be able to work with Marcel Thorø at Montpellier, he went to Nice, where he stayed for two years of domestic and political anxiety until his wife's death in 1940. He then avoided internment by escaping to Lisbon, and thence reached South Africa, where he became domiciled. For a time he took an active part in educational affairs in Southern Rhodesia and Natal, and here he met Mrs Joy Daisy Millard, whom he married in 1946. Shortly afterwards he moved to Capetown, where he worked with Dr Scafe, the Director of Marine Research, and gave a short course of lectures at the University. Then, with the advent of more settled conditions in Europe, Spencer and his wife returned and travelled in Switzerland, Italy and Austria before settling at Beaulieu in the Alpes Maritimes. Here he lived until 1953 (when his wife died) working on Ordovician starfishes at the University of Montpellier. He then returned to England and, apart from spending the winter of 1953-54 in France, lived at Ipswich with his daughter, Dr Hermione Egerton, until his death.

Spencer was a tall, impressive figure with grey piercing eyes and, in later years, bushy white hair framing a bald head. Although outwardly rather formidable in appearance, he was in reality a loveable and much beloved man, deservedly popular with his peers and subordinates alike. According to his own account, his chief recreations were golf, bridge and cooking. He claimed to make the best omelettes produced in England, though he admitted his failure to attain the highest French standards. Some measure of his enthusiastic capacity for work may be gathered from the fact that his greatest output of scientific research coincided in date with his most fruitful and successful activities in official work. He became a Fellow of the Geological Society in 1903 and received an award from the Lyell Fund in 1918. He was elected to the Royal Society in 1931. By his first marriage he had two daughters (both Doctors of Medicine) who survive him.

II. Professional work

As already noted, Spencer joined the Board of Education in 1904 as an Inspector of Schools, and remained in that service until his retirement in 1938. For the first ten years of his Inspectorship he was working in Whitehall; then, in 1914, he was appointed H.M.I. for Ipswich, East Suffolk and Lowestoft and also Chief Science Inspector for England and Wales. The following extract from an article in the *East Anglian Daily Times* of 6 October 1955, summarizes the nature of his work in the former capacity:

‘Dr. Spencer guided the Ipswich and East Suffolk educational programme for a quarter of a century, and in that time the district saw an unprecedented advance in educational development. Rural districts particularly benefited from this guidance, through the establishment of area schools.

‘When the Hadow Report on the reorganization of Council Schools was drawn up in 1926, Dr Spencer, with the Rev. J. T. R. Darling, then Chairman of the Education Committee, and Mr H. M. Spink, chief education officer, drew up a plan of reorganization for schools in East Suffolk. The County Council accepted it, and within two years the first school under the scheme was built.

‘Under Dr Spencer’s guidance, more schools followed in rapid succession, and the effect of the development spread so widely that in 1934 the Minister of Education published a pamphlet drawing attention to East Suffolk’s reorganized schools.’

An appreciation of Spencer’s work by Mr Henry Spink, a sometime colleague, contains the following more intimate details: ‘The years between 1926 and 1934 saw much new building in East Suffolk, and the new schools were the ancestors of the “senior modern” schools of today. In this work Spencer played a full part. He was actively enthusiastic, and the Authority was always acutely conscious that he would back their proposals. He abhorred red-tape, and brought a refreshing and engaging frankness to the whole enterprise.

‘He was large in size, and brought to his work a large conception. His approach was always friendly and he had the great quality of infecting others with his enthusiasm. He was beloved of all teachers—never unduly critical but invariably seizing upon some good quality of each and making each one feel twice the man he really was. He was a strong supporter of the plan for school meals, and even in 1930 most schools had their school-kitchens. His delight was to go into the kitchen and make himself an omelette for lunch—he was a skilled performer.

‘He had no method of working, except a happy gift of improvisation, which was strange in a man trained in scientific method. Yet in his approach to school-problems he had a knack of touching exactly the right keys. I have seldom seen him at fault. He properly distrusted the “committee-mind” and was a little aloof from administrative procedures.

‘I remember him with deep affection. He lectured very disjointedly—

almost to the point of exasperation. Yet at his most exasperating, one was likely to pick up pearls.'

In a series of autobiographical notes, written in a barely legible script with abbreviations for almost all the significant words, Spencer explains how he carried to his work the lessons he had learned at Batley, for he repeatedly emphasized the need for handling and studying material objects by school-children. He urged that they would learn more from experience than from verbal teaching. He had little patience with the examination system current in his day, and one shrinks from contemplating his reaction to modern developments of that system.

So remarkable was the success of the East Suffolk schemes in which Spencer took a leading part that it attracted admiring attention in educational circles in countries outside Britain. A final quotation, from an article by Dr Reinhold Schairer written for the *Frankfurter Zeitung* in 1933 will indicate the impression made on visiting observers. 'Mr Spencer is no dry pedant or bureaucrat. He is well aware that good in education can only come from widespread freedom from narrowing and officious interference. So, as soon as things are in motion, he leaves each group, each school, a freedom so large that continental onlookers are often astounded. Science is, next to horticulture and cooking, his greatest favourite. Booklore does not suffice: the hand, the rules of handicraft, are much more appropriate. Therefore he looks on this experiment with the calm yet benevolent eyes of a scientist who pursues a new line of research.'

III. Scientific work

Although his work on Palaeozoic starfishes is that for which Spencer will always be remembered, he wrote three short contributions to arthropod research. The first, the result of his early studies at Marburg under Professor E. Korshelt, was concerned with the central nervous system of the Phyllopoda with comments on the frontal organ. This work led him to support Lindstroem's view that the paired 'maculae' occurring on various trilobite hypostomes were ventrally situated degenerate compound eyes. In his comments on that author's work on trilobite hypostomes (1903), Spencer argued that the degenerate character of these 'eyes' disproved Patten's suggestion that *Branchipus* and *Limulus* were descended from the trilobites, and that all three had inherited the structures from a common ancestor. The validity of his arguments, logical though they were, is seriously impaired today by the generally held opinion that the 'maculae' are muscle-scars and had no sensory function. In the same paper Spencer advanced strong arguments in support of the belief that trilobites had much the same habits of life as the modern phyllopods. Although his contention that this was indicated by the fact that their remains are usually found lying on their backs sounds a little far-fetched, it illustrates a trend of thought that was to prove of great value in his later work on starfishes.

Dr C. J. Stubblefield writes 'Spencer retained his early interest in trilobites, and we often speculated together on the feeding mechanisms of different kinds: but he published nothing on the subject. He was, throughout the period of our friendship, avidly interested in the lessons to be learned from trilobite distribution in space and time, so that he might compare these with his own views on Asterozoan distribution. These mutual interests led to the holding at the 1938 meeting of the British Association of a symposium in section C entitled "The distribution and migration of certain animal groups in the British Lower Palaeozoic faunas". There were four contributions dealing respectively with trilobites, graptolites, brachiopods and starfishes. The last, given by Spencer, was not published until 1950, when it appeared in an expanded form in the *Geological Magazine*.'

The only other publication by Spencer that was concerned with the Arthropoda was a short notice, and a somewhat unconvincing illustration, of a reputed Eurypterid from the Bokkeveld Series of South Africa, issued in 1950. These equivocal fragments are considered by Stubblefield to be more likely those of trilobites.

Spencer's work on the Echinodermata began with a paper published in the *Proceedings of the Royal Society* in 1904. This dealt with the echinoid genus *Palaeodiscus* and the edrioasteroid *Agelacrinus*, and was obviously written under the inspiration, if not the direction, of W. J. Sollas. It may be regarded as the work of an apprentice to the craft of interpreting the remains of Palaeozoic echinoderms, and as such as marking a phase in Spencer's development. No one who has striven to disentangle the structures involved in the compressed casts of decalcified Silurian echinoderms can look on any efforts of that kind with anything but sympathy; and although later research has greatly modified Spencer's interpretation of the characters and systematic position of *Palaeodiscus*, it is impossible to regard his work otherwise than as a gallant attempt. Moreover, since *Palaeodiscus* is known only from the Silurian starfish-bed at Leintwardine, Spencer's interest in the 'Stelleroids' that are its companions may well have been aroused during the course of his study.

His next published work consisted of a completion of the monograph on the British Cretaceous Asterozoa left unfinished at the death of W. P. Sladen. This was a competent series of systematic descriptions that maintained, and even improved upon, the standard set in the earlier parts. In regard to that work, Spencer said that when it was finished he asked F. A. Bather what he thought of it. Bather deflated his pride by replying 'What conclusions have you drawn from your studies?' This remark certainly influenced Spencer's treatment of Palaeozoic starfishes, and may well have induced him to write his first really important and original paper on the 'Evolution of the Cretaceous Asterozoa' in 1913. This work is one of the greatest significance to palaeontologists and stratigraphers alike, for in it he not only discussed the biology of the starfishes, but showed that it is possible to recognize with considerable precision the horizon from which even an isolated ossicle has been collected.

It was in 1914 that the first part of Spencer's great 'Monograph on the Palaeozoic Asterozoa' appeared in the annual volume of the Palaeontographical Society, and this was followed in due course by nine other parts, amounting in all to 540 pages illustrated by 38 plates and many text-figures. He had prepared material for the completion of the monograph at the time of his death; but his notes were written in a manner that he alone could interpret, so that it may well be that the 'Monograph' will never be finished in the way that he intended. Fortunately for his successors, he produced in 1951 a summary of his conclusions on the bionomics and systematics of the earliest known starfishes, so that his 'Monograph', though unfinished in the technical sense, has been more or less rounded off.

It was perhaps inevitable that Spencer did little field-collecting, for his professional duties left him scant leisure for such work. In later years he visited localities in the Tremadocian and Lower Ordovician rocks of the Montagne Noir with Professor M. Thoräl; but most of the material described in his 'Monograph' came from public or private collections. By reason of his personality and reputation, material was entrusted to him from collections in this country and abroad, so that his acquaintance with his subject was as complete as possible.

In the study of material such as that presented by the Palaeozoic starfishes, the technique of preparation is at least as important as perspicacity in interpretation; and Spencer showed great ingenuity in extracting from his specimens all the structural details that they enclosed. Most of his material consisted of natural casts in decalcified sandstones and mudstones, and he became wonderfully skilful in making guttapercha moulds from the friable, and often deeply undercut, impressions. A large proportion of the figures on the plates of his 'Monograph' are photographs of such artificial moulds, in which the natural negatives are reproduced as positives. He was fortunate in having available the photographic skill of J. W. Tutchet, and even more so in employing the accurate artistry of F. J. Jones, whose wash-drawings make the text-figures in the 'Monograph' one of its most attractive and valuable features.

Spencer's 'Monograph' started with a preliminary statement in which he anticipated that his studies would 'cover the dry bones of these forms with flesh' and 'give a clearer picture of the ebb and flow of life in the dim past and provide tangible hypotheses concerning their evolution to be tested by future research'. How well his hopes were justified is manifest, not only in the pages of the 'Monograph', but in other papers, especially his last published in the *Philosophical Transactions* in 1951.

As Spencer worked on his 'Monograph', he became increasingly keen to understand the working mechanism of the various parts of the skeleton, especially with reference to the feeding-habits of the starfish. He constructed a model, now in the possession of the British Museum (Natural History), to help to explain the movements of the ophiuroid mouth-frame, and he made many plaster enlargements of other parts of the skeleton. He was often

convincingly successful in interpreting the mode of life of the starfish by an analysis of the attitude in which its remains are preserved.

The discovery by M. Thoral of Asterozoa in the Tremadocian of the Montagne Noir, the earliest undoubted forms known, naturally aroused Spencer's enthusiastic interest, and doubtless influenced his decision to live in the South of France on his retirement. He was able to prove that these primitive forms were devoid of the ventral groove that had been claimed as evidence of the derivation of starfish from edrioasteroids. He proposed a new subclass for the early forms, the Somasteroidea, of equal rank with the Ophiuroidea and Asteroidea. The Somasteroidea contain but one order, the Ophiuroidea two, and the Asteroidea three. This classification, the fulfilment of Spencer's promise made in the first part of his 'Monograph' that after detailed description of the available material, and not till then, a new taxonomic scheme would be attainable, has already been adopted by G. Ubaghs in the *Traité de Paléontologie*, edited by J. Piveteau. It will be amplified in the contribution virtually completed by Spencer that will appear in the *Treatise of Invertebrate Paleontology* now in course of publication under the joint auspices of the Geological and Paleontological Societies of America and the Palaeontographical Society.

In the compilation of this account of Spencer's life and work, the writer was given much help by his daughter Dr Hermione Egerton, by Sir M. P. Roseveare, by Messrs Lawson, Pitt and Spink, and by Dr C. J. Stubblefield, F.R.S. To all of these he expresses his sincere gratitude.

H. L. HAWKINS

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E. T. Whittaker

EDMUND TAYLOR WHITTAKER

1873-1956

LIFE

EDMUND TAYLOR WHITTAKER was born on 24 October 1873, the eldest child of John Whittaker and his wife Selina, daughter of Edmund Taylor, M.D. The family belonged to the district where the river Ribble forms the boundary between Lancashire and Yorkshire: his father, John Whittaker (1820-1910) was the youngest son of the first marriage of Henry Whittaker (1780-1853) of Grindleton near Clitheroe, who was the sixth of the eight sons of Richard Whittaker of Rodhill Gate near Grindleton. Several of Richard's descendants were men of distinction in the late Victorian period, among them the Right Hon. Sir Thomas Palmer Whittaker (1850-1919), for many years M.P. for the Spen Valley division of Yorkshire, and Sir Meredith T. Whittaker (1841-1931), of Scarborough.

His father on his marriage to his mother—whose father practised as a physician at Middleton near Manchester—settled in Southport. Though not well off, they were able to live on their private means: and there Whittaker was born. In childhood he was extremely delicate, and his time was spent almost entirely with his mother, who devoted herself to him and was in his earlier years his only teacher. As he grew older he grew stronger, and at the age of eleven was sent away from home to the Manchester Grammar School, which then dominated education in Lancashire. He was on the classical side, which meant that three-fifths of his time was devoted to Latin and Greek: in the lower forms, where the study was purely linguistic, he did well: but his lack of interest in poetry and drama caused a falling-off when he was promoted to the upper school, and he was glad to escape by electing to specialize in mathematics. Only after he had left school did he discover the field of Latin and Greek learning that really appealed to him—ancient and mediaeval theology, philosophy and science.

He gained an entrance scholarship to Trinity College, Cambridge, in December 1891, and was bracketed second wrangler in the mathematical tripos of 1895. Six Fellowships of the Royal Society were afterwards attained by the men of this tripos—T. J. Bromwich (analyst), J. H. Grace (geometer), A. Young (algebraist), B. Hopkinson (engineer), F. W. Carter (engineer), and himself. He was elected a Fellow of Trinity in October 1896, was put on the staff, and was awarded the First Smith's Prize in 1897.

On 7 August 1901 he was married to Miss Mary Boyd, daughter of the

Rev. Thomas Boyd, M.A., and granddaughter of Sir Thomas Jamieson Boyd of Edinburgh.

In 1906 Whittaker was appointed professor of astronomy in the University of Dublin, with the title of Royal Astronomer of Ireland. The Observatory attached to the Chair was situated at Dunsink, five miles from Dublin: its equipment was very poor, as there was no fund for the purchase of instruments, and in fact none had been bought for a generation past: it was tacitly understood that the chief function of the professor was to strengthen the school of mathematical physics in the University. He gave courses of advanced lectures, one of his pupils being Eamon de Valera.

In the spring of 1912 he was elected to the professorship of mathematics in the University of Edinburgh. Here he taught for thirty-four years, until his retirement in 1946. Their family of five was completed in Edinburgh by the birth of their youngest child, the two eldest having been born in Cambridge and the third and fourth at Dunsink.

In connexion with the Edinburgh chair, may be mentioned (1) the institution in 1914 of what was probably the first University mathematical laboratory and (2) the research school, many of whose past members are now distinguished men, including Whittaker's own successor, Dr A. C. Aitken, F.R.S.; and (3) the Edinburgh Mathematical Society, whose publications have a high reputation. Whittaker took a considerable share in administrative work, being Dean of the Arts Faculty from 1924 to 1927, and serving for two periods of office as a representative of the Senators on the University Court.

His house in George Square was a great centre of social and intellectual activity where liberal hospitality was dispensed to students of all ages. In this, as indeed in all his work, he was strengthened and supported by the gracious presence of Lady Whittaker. He was wonderfully happy in his home life and was greatly loved by all his family.

From other universities Whittaker received the honorary degrees of LL.D. (St Andrews, 1926, and California, 1934), Sc.D. (Dublin, 1906) and D.Sc. (National University of Ireland, 1939, and Manchester, 1944).

Some of his books and papers (particularly B.8, B.9, B.10 and B.11)* represent lectures or courses of lectures given at other universities. He held the Rouse Ball and Tarner lectureships at Cambridge (in 1926 and 1947, respectively), the Herbert Spencer lectureship at Oxford (1948), the Donnellan lectureship at Dublin (1946), the Riddell lectureship at Durham (Newcastle) (1942), the Selby lectureship at Cardiff (1933), the Hitchcock professorship at the University of California (1934), the Bruce-Preller lectureship of the Royal Society of Edinburgh (1931), the Larmor lectureship of the Royal Irish Academy (1948), and the Guthrie lectureship of the Physical Society (1943).

Whittaker was elected a Fellow of the Royal Society in 1905, served on the

* The references are to the Bibliography at the end of this notice, the letter B indicating the lists of Books and Monographs, and the letter R the list of Research Papers.

Council in 1911-12 and 1933-35, was awarded the Sylvester Medal in 1931 and the Copley Medal in 1954. With the Royal Society of Edinburgh he had continuous contact, being Gunning Prizeman in 1929 and President 1939-1944. At the end of his tenure of the Presidency, a bronze portrait head, executed by Mr Benno Schotz, R.S.A., was subscribed for by the Fellows and placed in the Society's House. He was President of the Mathematical Association in 1920-21, of the Mathematical and Physical Section of the British Association in 1927, and of the London Mathematical Society in 1928-29, being awarded its De Morgan Medal in 1935.

He was an Honorary Fellow or Foreign Member of the Accademia dei Lincei (1922), the Societa Reale di Napoli (1936), the American Philosophical Society (1944), the Académie Royale de Belgique (1946), the Faculty of Actuaries (1918), the Benares Mathematical Society (1920), the Indian Mathematical Society (1924), and the Mathematical Association (1935). Only twelve days before his death he was elected as a corresponding member of the French Academy of Sciences for the section of Geometry.

He was received into the Roman Catholic Church in 1930, and was Honorary President of the Newman Association (the society of Catholic graduates of Universities) in 1943-45. H.H. Pope Pius XI conferred on him the Cross *Pro Ecclesia et Pontifice* in 1935, and appointed him a member of the Pontifical Academy of Sciences in 1936.

Whittaker died on 24 March 1956. He is survived by his widow and all his five children.

WORK

1. Algebra

As a result of reading the first two volumes of Sir Thomas Muir's great work *The theory of determinants in the historical order of development*, Whittaker was stimulated in the years 1914-1922 to publish half a dozen papers on algebraical topics (R.22, R.28, R.29, R.31, R.33, R.34). Perhaps the most interesting of the theorems obtained was the following formula for the solution of algebraic equations (R.33). The root of the equation

$$0 = a_0 + a_1x + a_2x^2 + a_3x^3 + \dots$$

which is the smallest in absolute value is given by the series

$$\begin{array}{c} \frac{a_0}{a_1} - \frac{a_0^2}{a_1} \quad a_2 \quad - \quad \frac{a_0^3}{a_1} \quad a_2 \quad a_3 \quad - \quad \dots \\ \frac{a_1}{a_1} \quad \left| \begin{array}{cc} a_1 & a_2 \\ a_0 & a_1 \end{array} \right| \quad \left| \begin{array}{cc} a_1 & a_2 \\ a_0 & a_1 \end{array} \right| \quad \left| \begin{array}{ccc} a_1 & a_2 & a_3 \\ a_0 & a_1 & a_2 \\ 0 & a_0 & a_1 \end{array} \right| \quad \dots \end{array}$$

	a_2	a_3	a_4	
a_0^4	a_1	a_2	a_3	
	a_0	a_1	a_2	
		a_1	a_2	a_3
			a_2	a_3
				a_3

2. Interpolation

A small group of papers owe their existence to the fact that Edinburgh is one of the chief centres of Life Assurance. Whittaker formed friendships with many of the actuaries engaged in it, who told him of various mathematical problems that had arisen in their experience, mostly relating to interpolation, curve-fitting, and probability. Becoming interested in the theory of interpolation, he attempted to answer some of its fundamental questions.

If the values of an analytic function $f(x)$ which correspond to a set of equidistant values of its argument (say $a, a+w, a-w, a+2w, a-2w, a+3w, \dots$) are known, then we can write out a 'table of differences' for the function. All the analytic functions which give rise to the same difference-table may be said to be *cotabular*; any two cotabular functions are equal to each other when the argument has any one of the values, $a, a+w, a-w, \dots$ but they are not in general equal to each other when the argument has a value not included in this set.

In the theory of interpolation certain expansions are introduced in order to represent the function $f(x)$, for general values of x , in terms of the quantities occurring in the difference-table: for example, the Newton-Gauss formula

$$f(x) = (fa) + \frac{x-a}{w} \Delta f(a) + \frac{(x-a)(x-a-w)}{2!w^2} \Delta^2 f(a) + \dots$$

There is no reason *a priori* why this expansion should represent $f(x)$ in preference to any other function of the set cotabular with $f(x)$; and thus two questions arise, namely:

- (1) Which one of the functions of the cotabular set is represented by the Newton-Gauss expansion?
- (2) Given any one function $f(x)$ belonging to the cotabular set, is it possible to construct from $f(x)$ a simple analytical expression for that function of the cotabular set which is represented by the Newton-Gauss expansion?

These questions were answered in a paper published in 1915 (R.27). It was shown that there exists a function which belongs to the cotabular set, and which, when subjected to Fourier analysis, has no periodic constituents of period less than twice the tabular interval w : and that this function is uniquely defined by the set. He called it the *cardinal function* of the set. Then the answer to the first question is that the function represented by the Newton-Gauss expansion is the cardinal function: the answer to the second question is that the cardinal function is represented by the series

$$\sum_{n=-\infty}^{\infty} \frac{f(a+nw) \sin \left\{ \frac{\pi}{w}(x-a-nw) \right\}}{\frac{\pi}{w}(x-a-nw)}$$

Some years later Whittaker discussed (R.39, R.40) another problem which was derived from his actuarial friends, namely that of *graduation*: this may be described as follows. A set of numbers $u_1, u_2, u_3, \dots, u_n$ is supposed to have been obtained from observations or statistics of some kind. These numbers would represent the values of a variable u_x corresponding to the values $1, 2, 3, \dots, n$ of its argument x , were it not that they are affected by accidental irregularities due to errors of observation, or to the imperfections of statistics. If we form a table of the differences $\Delta u_1 = u_2 - u_1$, $\Delta u_2 = u_3 - u_2$, $\Delta^2 u_1 = \Delta u_2 - \Delta u_1$, etc., it will often be found that these differences are so irregular that the difference-table cannot be used for the purposes to which a difference-table is usually put; before we can use the difference-table, we must perform a process of 'smoothing', that is to say, we must find another sequence, $u'_1, u'_2, u'_3, \dots, u'_n$, whose terms differ as little as possible from the terms of the sequence, $u_1, u_2, u_3, \dots, u_n$, but which has regular differences. This smoothing process is called the *graduation* or *adjustment* of the observations.

Various ways of performing it were described in the actuarial text-books, but these had no logical basis. The standpoint which Whittaker now proposed was to recognize that the problem belonged essentially to the mathematical theory of probability, and that a logical solution could be found for it. To effect this, he took as a measure of the *smoothness* of the graduated values, the sum of the squares of their third differences,

$$S = (u'_4 - 3u'_3 + 3u'_2 - u'_1)^2 + (u'_5 - 3u'_4 + 3u'_3 - u'_2)^2 + \dots,$$

while as a measure of the *fidelity* of the graduated to the ungraduated values he took the sum of the squares of their difference

$$F = (u_1 - u'_1)^2 + (u_2 - u'_2)^2 + \dots + (u_n - u'_n)^2.$$

Then by an application of the theory of inductive probability, he showed that

the most probable set $(u_1', u_2', \dots, u_n')$ of graduated values is that which makes

$$\lambda^2 S + F$$

a minimum, where λ is a constant which expresses the degree to which we are justified in sacrificing fidelity in order to obtain smoothness. On the basis of this rule, the practical details of performing the graduation were worked out. The method has the advantage that the total of the u 's and their first and second moments are the same in the graduated table as in the actual statistics on which it was based.

3. Automorphic functions

Whittaker's first long paper, 'On the connexion of algebraic functions with automorphic functions', (R.2) was the definitive edition of his Trinity fellowship thesis and Smith's Price essay. If u is a *many-valued* function of z , defined by an equation $f(u, z) = 0$, then it may be possible to find a variable t , such that both u and z are *one-valued* functions of t ; t is then called a *uniformizing variable* for the equation $f(u, z) = 0$. The simplest many-valued functions are the *algebraic* functions; for these Poincaré and Klein have proved a general existence theorem, namely, that if u is an algebraic function of z , then a uniformizing variable t exists, and u and z are *automorphic* functions of t ; automorphic functions are the natural generalization of circular and elliptic functions. The existence theorem however did not connect t analytically with u and z .

Now algebraic functions are classified according to their *genus*: and when the genus is zero or unity, t can be found, for the automorphic functions required are rational functions and doubly periodic functions, in the two cases respectively; but no class of automorphic functions with simply connected regions (analogous to period parallelograms) had been found hitherto, which was applicable to the uniformization of algebraic functions whose genus is greater than unity. In the paper under review, a new class of discontinuous groups of transformations of a variable t was obtained as a subgroup of the group generated by elliptic transformations each of period 2 i.e. substitutions of the form

$$\frac{t' - \bar{\alpha}}{t' - \alpha} = - \frac{t - \bar{\alpha}}{t - \alpha},$$

α and $\bar{\alpha}$ being the conjugate double points. A method was given for dividing the plane into regions which are transformed into one another, by the transformations of such a 'Whittaker group'. The regions are curvilinear polygons whose sides may be regarded as straight lines in the sense of non-

Euclidean geometry, with the 'distance' between two points A and B specified by the integral,

$$\int \frac{|du + idv|}{v}$$

taken along the circle orthogonal to the real axis and joining A and B . The scheme is therefore analogous to the familiar partition of the plane into period parallelograms. It was shown that the automorphic functions of a Whittaker group can affect the uniformization of algebraic functions of any genus whatever.

If $f(u_1z)$ is an algebraic form of genus 2 it can be brought by a birational transformation into the normal form

$$u^2 = (z - e_1)(z - e_2)(z - e_3)(z - e_4)(z - e_5),$$

and Whittaker proved that the uniformizing variable t for this equation is the quotient of two solutions of a linear differential equation of the form

$$\frac{d^2y}{dz^2} + \frac{3}{16} Iy = 0,$$

where

$$I = \sum_{n=1}^5 \frac{1}{(z - e_n)^2} - \frac{4z^2 + c_1z^2 + c_2z + c_3}{(z - e_1)(z - e_2)(z - e_3)(z - e_4)(z - e_5)},$$

where c_1 , c_2 and c_3 are constants whose values were unknown.

In a later paper (R.49) Whittaker conjectured that if the normal form of the algebraic equation of genus 2 is written as $s^2 = f(z)$, then the correct form of the differential equation is

$$\frac{d^2y}{dz^2} + \frac{3}{16} \left\{ \frac{f'^2}{f^2} - \frac{6}{5} \frac{f''}{f} \right\} = 0,$$

in which all the constants are completely determined. If $s^2 = z^5 + 1$, this equation can be integrated in terms of hypergeometric functions, and the conjecture was triumphantly verified.

A little later J. M. Whittaker (*J. Lond. Math. Soc.* **5**, 1930, p. 150) proved that the conjecture is universally true for the form $u^2 = f(z) = (z - e_1) \dots (z - e_n)$ if the invariant I in the differential equation has the form

$$I = \sum \frac{1}{(z - e_i)^2} - \frac{p(z)}{f(z)},$$

where $p(z)$ is a polynomial.

A minor but interesting contribution (R.7) to the theory of automorphic

functions gives an explicit formula for a function which is analogous to Weierstrass's sigma-function in the form

$$\sigma(z, \alpha) = (z - \alpha) \prod_{n=1}^{\infty} (1 - c_n) \exp \frac{1}{2} c_n,$$

where

$$c_n = \frac{(z - \alpha)(z_n - \alpha_n)}{(z - z_n)(\alpha - \alpha_n)},$$

and $z_n = (a_n z + b_n)/(c_n z + d_n)$ is a typical transformation of the group.

4. Astronomy

In 1899, Whittaker prepared for the British Association a monumental 'Report on the progress of the solution of the problem of three bodies' (B.1) which covers the period 1868-1898, and which provides a complete summary of the development of the 'new dynamical astronomy'.

While Whittaker was Astronomer Royal of Ireland he wrote a few papers on various astronomical subjects.

A short note in 1906 (R.15) gives a simple and completely general proof that the resolving power of any dispersive apparatus is equal to the number of separate pulses into which an incident pulse is broken up by the apparatus.

Another brief communication in 1908 (R.17) marshals the evidence then available that the stratum of the solar atmosphere, which gives rise to the selective and general absorption characteristic of the spectrum of sun spots, is a region of high pressure and density rather than of high temperature.

There are only two papers on observational astronomy. The first (R.19) analyzes the light curve of the variable star RW Cassiopeiae; while the second (R.20) discusses the variations in the irregular variable SS Cygni with a view to determining possible periodicities.

5. Potential theory and special functions

Perhaps the most important section of Whittaker's researches is that which contains the magnificent series of papers on the special functions of mathematical physics regarded as constituents of potential functions.

Towards the end of 1902 Whittaker communicated a note to the Royal Astronomical Society (R.8), later expanded into a substantial paper and published in *Math. Ann.* (R.10), on the general solution of Laplace's equation

$$\frac{\partial^2 V}{\partial x^2} + \frac{\partial^2 V}{\partial y^2} + \frac{\partial^2 V}{\partial z^2} = 0,$$

which was obtained in the form

$$V = \int_0^{2\pi} f(x \cos \alpha + y \sin \alpha + iz, \alpha) d\alpha,$$

where f is an arbitrary function of its two arguments $(x \cos \alpha + y \sin \alpha + iz)$ and α . This result brought a new unity into potential-theory, since all harmonics must be expressible in this form, and in fact the expression of an arbitrary solution of Laplace's equation as a series of spherical, cylindrical harmonics, etc., is equivalent to the expansion of the function f of the general solution into power-series, Fourier series, Dirichlet series, etc., with respect to each of its arguments.

Following on the solution of Laplace's equation, two other papers on analysis were written in 1903. The first (R.11) was a study of the functions defined by the differential equation

$$\frac{dy}{dz^2} + \mathcal{Z}y = 0$$

where $\mathcal{Z}$ is a quadratic function of z . These he named *functions of the parabolic cylinder*: a standard solution denoted by $D_n(z)$ was obtained, and various representations and properties were found. In the second paper (R.12) it was shown that many special functions which had been introduced at different times by various authors were particular cases of a more general function which he denoted by $W_{k,m}(z)$, and for which he obtained various integral properties, asymptotic expansions, recurrence-formulae, etc. When the second edition of *Modern analysis* was published in 1915, Whittaker and Watson resolved to devote an entire chapter to $W_{k,m}(z)$, and a name had to be found for it: after some discussion, they settled on the *confluent hypergeometric* function. It has been found useful in applications, and an extensive literature has grown up.

In 1912 Whittaker returned to the topic of special functions with a paper (R.21) on the solutions of the linear differential equations of the second order with periodic coefficients,

$$\frac{d^2y}{dz^2} + (a + k^2 \cos^2 z)y = 0,$$

where a and k are constants. This equation, which occurs frequently in mathematical physics, and in connexion with Hill's lunar theory, is of interest because it is (from the point of view of the classification of differential equations according to their singularities) the simplest linear differential equation which is not reducible to a particular or degenerate case of the hypergeometric differential equation. Its solutions are not, in general, periodic functions of z ; but there are an infinite number of solutions which are periodic functions of z , of period 2π . It was with these (which are the solutions

of importance in physics) that the paper under review was concerned: Whittaker called them *Mathieu functions*, and the differential equation *Mathieu's equation*, in honour of Mathieu, who discovered them in 1868: and these names have been generally accepted. The chief result now published was that the Mathieu functions satisfy a homogeneous integral-equation, namely

$$y(z) + \lambda \int_0^{2\pi} \exp(k \cos z \cos \theta) y(\theta) d\theta = 0.$$

This integral-equation does not possess a solution except when λ has one of a certain set of values: and the solutions corresponding to these values of λ are precisely the periodic solutions of the differential equation, that is to say, they are the Mathieu functions required in physics. It was shown how explicit expressions for them can be obtained by solving the integral-equation.

The integral-equation plays much the same part in the theory of Mathieu functions that Bessel's formula

$$\mathcal{J}_n(z) = \frac{1}{\pi} \int_0^\pi \cos(ns - z \sin s) ds$$

does in the theory of Bessel functions. This is an instance of a general theorem which he proved later, namely that integral-equations play the same part in relation to differential equations with *four* regular singularities, that ordinary definite integrals do in relation to differential equations with *three* regular singularities.

This work on Mathieu functions was essentially an application of the 1902 solution of Laplace's equation: the integral-equation had in fact been given to his advanced pupils in Cambridge in 1904, but at that time he appreciated its significance so little that he did not publish it. His pupil, H. Bateman, mentioned it in 1909, ascribing it to Whittaker (*Trans. Camb. Phil. Soc.* **21**): meanwhile it was independently discovered by the Japanese mathematician K. Aichi in 1908.

The general solution of Mathieu's equation is of the form,

$$y = Ae^{\mu z} \phi(z) + Be^{-\mu z} \psi(z),$$

where A and B are arbitrary constants, μ is a constant depending on the constants a and k of the differential equation, while $\phi(z)$ and $\psi(z)$ are periodic functions of z . For certain values of a and k the constant μ vanishes, and the solution is then one of the purely-periodic functions already referred to: but in general μ is different from zero; a paper of 1914 (R.23) was devoted to its determination.

One of the research students who was working with Whittaker in Edinburgh at this time, E. L. Ince, never lost interest in Mathieu's equation, and

did much original work of the highest quality on it many years afterwards. Whittaker himself returned to it in 1928 (R.48), obtaining the property of the Mathieu functions which corresponds to the property (possessed by all functions of the hypergeometric family) of satisfying *recurrence-formulae*.

The work on the integral-equation satisfied by the Mathieu functions led to papers on integral-equations satisfied by the other functions of mathematical physics. It was shown (R.24, R.25) that the functions of Lamé, that is to say, the doubly-periodic solutions of the differential equation

$$\frac{d^2y}{dz^2} = \{n(n+1)k^2\operatorname{sn}^2 z + A\}y,$$

are the solutions of the homogeneous integral-equation

$$y(z) = \lambda \int_0^{4K} P_n(k \operatorname{sn} z \operatorname{sn} s) y(s) ds,$$

where P_n is Legendre's polynomial, and k is the modulus of the elliptic functions, whose period is $4K$.

This theorem was applied in the latter part of the paper in order to express the ellipsoidal harmonics (i.e. the solutions of Laplace's equation which are appropriate to the ellipsoid) in the form which he had found in 1902 for the general solution of Laplace's equation. He now showed that any ellipsoidal harmonic can be expressed in the form

$$\int_0^{2\pi} P_n \left(\frac{x \cos \theta + y \sin \theta + iz}{\sqrt{(z^2 - b^2 \sin^2 \theta)}} \right) f(\theta) d\theta,$$

where P_n denotes Legendre's polynomial: or, fixing the function $f(\theta)$ more precisely, any ellipsoidal harmonic can be expressed in the form

$$\int_0^{4K} P_n \left\{ \frac{1}{k'c} (k'x \operatorname{sn} s + y \operatorname{cn} s + iz \operatorname{dn} s) \right\} E(s) ds,$$

where $E(s)$ denotes a Lamé function.

A further paper of 1914 (R.26) introduced a class of functions which bear the same relation to the Mathieu functions that the associated Legendre functions $P_n^m(z)$ bear to the Bessel function $J_m(z)$. The differential equation of these functions is

$$\frac{d^2y}{dz^2} + \left\{ A - (n+1)l \cos 2z + \frac{1}{8}l^2 \cos 4z \right\} y = 0,$$

which is a case of Hill's equation in the lunar theory: and it was shown that

the periodic solutions of this equation are the solutions of the homogeneous integral-equation

$$y(z) = \lambda \int_0^{2\pi} \cos^n(y-s) \exp \frac{1}{2} l(\sin^2 s + \sin^2 z) y(s) ds.$$

The culmination of the series of papers on the solution of differential equations by definite integrals and by integral-equations was reached some years later in the following theorem (R.50) which seems to comprehend most of the known results in this field of analysis, and relates to partial as well as ordinary differential equations. Consider any contact-transformation from a set of variables $(q_1, q_2, \dots, q_n, p_1, p_2, \dots, p_n)$ to a set of variables $(Q_1, Q_2, \dots, Q_n, P_1, P_2, \dots, P_n)$. Suppose that the latter when expressed in terms of the former are denoted by $Q_r(q_1, q_2, \dots, q_n, p_1, p_2, \dots, p_n)$, $P_r(q_1, q_2, \dots, q_n, p_1, p_2, \dots, p_n)$, $(r=1, 2, \dots, n)$. Then the set of partial differential equations

$$\left. \begin{aligned} Q_r \left(q_1, q_2, \dots, q_n, \frac{\partial}{\partial q_1}, \frac{\partial}{\partial q_2}, \dots, \frac{\partial}{\partial q_n} \right) \chi &= t_r \chi, \\ P_r \left(q_1, q_2, \dots, q_n, \frac{\partial}{\partial q_1}, \frac{\partial}{\partial q_2}, \dots, \frac{\partial}{\partial q_n} \right) \chi &= -\frac{\partial \chi}{\partial t_r}, \end{aligned} \right\} (r = 1, 2, \dots, n),$$

is a compatible set of differential equations, and possesses a solution $\chi(q_1, q_2, \dots, q_n, t_1, t_2, \dots, t_n)$. Now let it be required to solve a set of n compatible linear partial differential equations in n independent variables, say

$$F_r \left(q_1, q_2, \dots, q_n, \frac{\partial}{\partial q_1}, \frac{\partial}{\partial q_2}, \dots, \frac{\partial}{\partial q_n} \right) \psi = 0 \quad (r = 1, 2, \dots, n), \quad (A)$$

where $\psi(q_1, q_2, \dots, q_n)$ is the function to be determined. Suppose that when the q 's and p 's are replaced by their values in terms of the Q 's and P 's, the function $F_r(q_1, q_2, \dots, q_n, p_1, p_2, \dots, p_n)$ becomes $G_r(Q_1, Q_2, \dots, Q_n, P_1, P_2, \dots, P_n)$. Then the solution of the partial differential equations (A) is furnished by definite integrals of the type

$$\begin{aligned} &\psi(q_1, q_2, \dots, q_n) \\ &= \iiint \dots \int \chi(q_1, q_2, \dots, q_n, t_1, t_2, \dots, t_n) q(t_1, t_2, \dots, t_n) dt_1 dt_2 \dots dt_n \end{aligned}$$

where $q(t_1, t_2, \dots, t_n)$ is the solution of the set of n compatible linear partial differential equations

$$G_r \left(t_1, t_2, \dots, t_n, \frac{\partial}{\partial t_1}, \frac{\partial}{\partial t_2}, \dots, \frac{\partial}{\partial t_n} \right) q = 0 \quad (r = 1, 2, \dots, n).$$

Among the applications of this theorem worked out in the paper were several

new properties of the hypergeometric function, the solution of the ordinary linear differential equation of the third order with four regular singularities, and an extension to partial differential equations of Laplace's method for solving ordinary differential equations.

6. Dynamics

Two early papers (R.1 and R.3) are devoted to a new method of obtaining the equation of planetary theory by the use of Lagrange brackets, and to a general process for reducing the order of the differential equations of a dynamical system by means of the integral of energy. Paper (R.18) discusses the vibrations of a compound system formed of two equal sub-systems which are coupled together by a connexion of 'gyroscopic' type. It was shown that the actual frequencies of such a compound system (*not* the *squares* of the frequencies, which are the quantities usually occurring in the solution of dynamical problems) can be expressed by a simple formula of Deslandres' type,

$$n = p^2 f(q^2, r^2) + Bq^2 + \phi(r^2)$$

in terms of numbers, p, q, r , which assume in succession all integer values, so that the spectrum of frequencies is of the 'banded' type.

But Whittaker's principal contributions to analytical dynamics are his well-known treatise (discussed below), his theory of periodic orbits and his elucidation of planetary theory by means of the 'adelphic integral'.

In (R.4) a new method of detecting periodic orbits was introduced in terms of the sign taken by the function

$$I = \frac{h - f(x, y)}{\rho} - \frac{1}{2} \cos \gamma \frac{\partial f}{\partial x} - \frac{1}{2} \sin \gamma \frac{\partial f}{\partial y}$$

on certain closed curves C . Here $f(x, y)$ and h are the potential energy at (x, y) and the total energy, ρ is the radius of curvature of C , and $(\cos \gamma, \sin \gamma)$ the direction cosines of the normal to C . It was shown that if one closed curve C_+ is contained within another closed curve C_- , and if I is positive at all points of C_+ and negative at all points of C_- , then in the ring shaped region between the two curves there exists a periodic orbit of the dynamical problem. Furthermore it was proved that the value of the integral

$$\frac{1}{2\pi} \iint \left\{ \left(\frac{\partial^2}{\partial x^2} + \frac{\partial^2}{\partial y^2} \right) \log \{h - f(x, y)\} \right\} dx dy$$

taken over the interior of any periodic orbit, is equal to the number of centres of force enclosed by the orbit, diminished by two. A subsequent paper (R.6) extended this result to periodic orbits described under *moving* centres of force and, in particular, to the restricted problem of three bodies.

Whittaker's classical researches on the integration of a system of Hamiltonian equations open in 1902 with a paper (R.5) which describes a process of successive transformations which yields a solution in terms of trigonometric series, the first terms of which correspond to a position of stable equilibrium and to small oscillations about its position. This work was completed in 1916 by the discovery of the 'adelphic' integral of a Hamiltonian system (R.30). In this paper Whittaker examined the difficulties indicated by Poincaré's celebrated theorem that the series of celestial mechanics, if they converge at all, cannot converge uniformly for all values of the time on the one hand, and on the other hand for all values of the constants comprised between certain limits.

The ideas of the paper may be illustrated most readily by considering a particular problem, namely that of the motion of a particle on the surface of a smooth ellipsoid under no external forces. The particle describes a geodesic on the surface, so the periodic solutions are simply those geodesics which are closed curves. Now a geodesic on an ellipsoid satisfies the equation

$$pd = \text{constant}$$

where p denotes the perpendicular from the centre of the ellipsoid on the tangent-plane at the point, and d is the diameter parallel to the tangent to the geodesic at the point. The same equation holds for the lines of curvature on the ellipsoid: so that every geodesic may be associated with a line of curvature, namely, that line of curvature for which pd has the same value as it has for the geodesic: and we may speak of the geodesic as 'belonging to' the line of curvature. There is only one line of curvature having a prescribed value for pd , but there is an infinite number of geodesics having this value for pd , so that an infinite number of geodesics belong to each line of curvature. The line of curvature consists of two closed curves on the ellipsoid: the region between these two portions of the line of curvature is a belt extending round the ellipsoid: and all the geodesics which belong to the line of curvature are comprised within this belt, and touch the two portions of the line of curvature alternately.

In order that the geodesic may be closed, it is necessary (as in all poristic problems) that a certain parameter (depending in this case on the value of the constant pd of the line of curvature) should be a rational number: the geodesic is unclosed if this parameter is an irrational number. If it is closed, then there are ∞^1 other geodesics which belong to the same line of curvature and which are also closed; but if it is not closed, then no other geodesic belonging to this particular line of curvature can be a closed geodesic.

Now consider the connexion between the ∞^1 members of the family of geodesics which belong to the same line of curvature. It is well known that if we have a dynamical system with two degrees of freedom, and if we denote the generalized co-ordinates by (q_1, q_2) and the momenta by (p_1, p_2) , and if

$$\phi(q_1, q_2, p_1, p_2) = \text{constant}$$

is an integral of the system, then the infinitesimal contact-transformation which is defined by the equations

$$\delta q_1 = \epsilon \frac{\partial \phi}{\partial p_1}, \quad \delta q_2 = \epsilon \frac{\partial \phi}{\partial p_2}, \quad \delta p_1 = -\epsilon \frac{\partial \phi}{\partial p_1}, \quad \delta p_2 = -\epsilon \frac{\partial \phi}{\partial p_2},$$

(where ϵ is a small constant) transforms any trajectory into an adjacent curve which is also a trajectory. If we apply this theorem to the motion on the ellipsoid, we find that the infinitesimal transformation which corresponds to the integral $pd = \text{constant}$ transforms any geodesic into another geodesic which belongs to the same line of curvature. Thus the ∞^2 geodesics on an ellipsoid may be classified into ∞^1 families, each family consisting of geodesics: the members of any one family are either all closed or all unclosed, and a certain continuous group of transformations, which is associated with the integral $pd = \text{constant}$, transforms any geodesic into all the geodesics which belong to the same family. Features essentially the same as these are to be found quite generally in dynamical systems with two degrees of freedom which possess an integral of energy. There exists an integral of the system (called by Whittaker the *adelphic integral*) which plays the same part as the integral $pd = \text{constant}$ does in the case of the ellipsoid: namely the infinitesimal transformation associated with it changes every trajectory of the system into an adjacent trajectory, in such a way that every periodic solution is changed into an adjacent periodic solution having the same period and the same constant of energy. This led to a method for finding the adelphic integral, and became the basis from which to complete the integration of the system. It appeared that although the adelphic integral can always be found (as an infinite series), it cannot in general be represented by a single analytical expression which is valid for all values of the parameters involved in the problem: its form is different in different cases, depending on whether a certain parameter has a rational or irrational value. This fact is the underlying reason for the difficulty formulated in Poincaré's theorem: for as the parameter changes continuously, it is perpetually passing through rational and irrational values, and at each change from rational to irrational, or *vice versa*, the form of the adelphic integral is abruptly changed; so that no one series-solution of the dynamical problem can be valid over the whole range. But when the rationality or irrationality of the parameter is given, the adelphic integral can be constructed and the solution can then be completed: so that Poincaré's difficulty is overcome.

7. Relativity and electromagnetic theory

Whittaker's first paper on theoretical physics (excluding dynamics) was published in 1904 (R.13). This paper shows that any electromagnetic field can be specified in terms of the derivatives of *two* real scalar wave functions, F and G , by means of the formulae,

$$\begin{aligned}\mathbf{E} &= \text{curl curl } \mathbf{F} + \text{curl } c^{-1} \dot{\mathbf{G}} \\ \mathbf{H} &= \text{curl } c^{-1} \dot{\mathbf{F}} - \text{curl curl } \mathbf{G} \\ \mathbf{F} &= (0, 0, F) \quad , \quad \mathbf{G} = (0, 0, G)\end{aligned}$$

(The usual specification in terms of the Maxwellian scalar-potential ϕ and vector-potential A is equivalent to *four* real functions connected by the relation $\dot{\phi} + c \text{ div } \mathbf{A} = 0$.)

These two scalar wave-functions were evaluated in terms of the charges and co-ordinates of the electrons generating the field.

From 1921 onwards he published a succession of papers on relativity. The first of them (R.36) was a relativistic generalization of Faraday's theory of lines and tubes of force. Surfaces in four-dimensional space-time were introduced which reduce to Faraday's tubes of electric force when the field is purely electrostatic and which reduce to Faraday's tubes of magnetic force when the field is purely magnetostatic, and which between these extreme types provide a continuous transition. The way in which the generalization is made may be illustrated by the following statement. With Faraday's electrostatic tubes, as we proceed along a tube, the magnitude of the electric vector at any point is inversely proportional to the area of the cross section of the tube, and the three components of the electric vector are to each other in the same ratio as the areas of the projections of this cross-section on the three co-ordinate planes. With the surfaces introduced in the paper under review (to which Whittaker gave the name *calamoids*), the quantity

$$\sqrt{\{(\text{electric vector})^2 - (\text{magnetic vector})^2\}}$$

(which, as is well known, is covariant with respect to all Lorentz transformations) is inversely proportional to the area of the cross-section of the calamoid, and the six components of the electric and magnetic forces are connected in a simple way with the areas of the six projections of this cross-section on the six co-ordinate planes of yz , zx , xy , xt , yt , zt . The calamoids are covariant with respect to Lorentz transformations, so they are the same, whatever be the observer whose measure of electric and magnetic force are used in constructing them.

This was followed by four other papers (R.44, R.45, R.46, R.47) concerned with electromagnetic phenomena in the curved space of general relativity. The simplifying assumption was made, that the gravitational field is statical, so that the curvature of space at any point does not vary with the time. In the distorted space of this fixed gravitational field, Whittaker supposed an electromagnetic field to exist: strictly speaking, the electromagnetic field has itself a gravitational effect, but this effect is in general small and he treated the ideal case in which it is ignored, so the metric is simply that of the gravitational field originally postulated. The problem is, therefore, to study

the existence and propagation of electromagnetic fields in a medium whose properties (i.e. the distortion of space) vary from point to point: and Whittaker showed that the problem is mathematically identical with that of the Maxwellian electromagnetic field in a medium whose dielectric constant and magnetic permeability have a particular kind of anisotropy. Many special problems were studied, such as the capture and imprisonment of radiation by the intense gravitational field surrounding a point-mass, the form of the equipotential surfaces due to an electric charge which is at rest in a gravitational field, and the expression of the electro-magnetic four-vector potential due to electrons moving in any manner. It was found that an electron at rest in a varying gravitational field must emit radiation: and that any electromagnetic disturbance which is filamentary must necessarily have the form of a null geodesic of space-time.

In 1931 he discussed (R.51) the definition of distance in the curved space of General Relativity, and the displacement of the spectral lines of distant sources. There are certain ambiguities involved in the use of the terms 'time', 'spatial distance', and 'velocity', when applied by an observer to an object which is remote from him in curved space-time. The 'interval' which is defined by $ds^2 = \Sigma g_{pq} dx^p dx^q$ involves space and time blended together: and although any particular observer at any instant perceives in his immediate neighbourhood an 'instantaneous three-dimensional space', yet this space cannot be defined beyond his immediate neighbourhood. The concept of 'spatial distance between two material particles' is, however, not really dependent on the concept of 'simultaneity'. When the astronomer asserts that 'The distance of the Andromeda nebula is a million light-years', he is stating a relation between the world-point occupied by ourselves at the present instant and the world-point occupied by the Andromeda nebula at the instant when the light left it which arrives here now: that is, between two world-points which lie on the same null geodesic. In order to define 'spatial distance' conformably to this idea, in a general Riemannian metric, Whittaker translated into the language of differential geometry the principle by which astronomers actually calculate the 'distance' of very remote objects such as the spiral nebulae, and so arrived at the following definition. He took a 'star' A and an observer B , which are on the same null geodesic (so that a ray of light can pass from A to B) and considered a thin pencil of null geodesics which issue from A and pass near B . This pencil intersects the observer B 's 'instantaneous three-dimensional space', giving a two-dimensional cross-section: the 'spatial distance AB ' is then defined to be proportional to the square root of this cross-section. Distance, as thus specified, is independent of the choice of the co-ordinate system. On the basis of this definition, an analytical theory was worked out and applied to the displacement of the spectral lines of distant sources and to the history of the observation of a star in the de Sitter world.

In 1935 (R.53) he extended to general relativity the well-known theorem of Gauss on the Newtonian potential, namely that the total flux of gravita-

tional force through a simple closed surface is equal to $(-4\pi) \times$ the total gravitating mass contained within the surface. In the extended theorem, the Newtonian concept of 'gravitating mass' is replaced by that of the energy-tensor, which does not in general consist solely of the 'material' energy-tensor and need not involve any 'matter' at all. The concept of the 'potential energy' of a particle in a statical field in general relativity was then elucidated. In Newtonian dynamics the equation of conservation of energy is

$$(\text{kinetic energy}) + (\text{potential energy}) = C$$

where the potential energy is (for a single particle) the product of the mass—a fixed quantity—into a function which depends only on the position of the particle, and where C is a constant which depends on the initial circumstances. In general relativity with statical fields on the other hand, the equation of conservation of energy for a single particle of proper-mass m is of the form

$$(\text{kinetic energy}) = (\text{lost potential energy})$$

where now the lost potential energy is the product of $m(1 - w^2/c^2)^{-\frac{1}{2}}$ into a function which depends only on the position of the particle, w denoting the velocity which the particle would have after escaping from the gravitational field and arriving at the Galilean space-time at infinity: the constant w corresponds to the Newtonian constant C , but enters into the equation in a wholly different manner. He called $m(1 - w^2/c^2)^{-\frac{1}{2}}$ the *potential mass*, and showed that this definition of potential mass enables us to express the generalized Gauss theorem, in the case when the energy-tensor is due to actual masses, by a simple statement almost identical with the original Gauss theorem of Newtonian theory. Finally it was shown that the electrostatic form of Gauss theorem in classical physics, namely that the total strength of the tubes of force issuing from a closed surface is equal to the total electric charge within the surface, can also be extended to general relativity, but that this extension is different in character from the gravitational theorem.

8. *Quantum theory*

Whittaker wrote a few papers on quantum theory, three of which show how certain characteristic quantum phenomena can be exhibited by classical systems. In (R.37) a magnetic structure was described, towards which an electron was supposed to be moving, and the encounter between them was investigated. It was found that if the initial velocity of the electron is less than a certain quantity, the electron, after advancing to a finite distance from the structure, comes to a stop and is then repelled, returning to its original position without any permanent exchange of energy: the electron and the structure have had an 'elastic impact'. If however the electron has a sufficiently great initial velocity, it is able to pass completely through and out of the structure, so as ultimately to be free from its influence: and in the process

it gives up a certain definite amount of energy to the structure, and retains the rest. The other two papers (R.41) and (R.42) provide models of light quanta in the form of exact solutions of Maxwell's equations, suitably generalized to include magnetic currents or magnetic dipoles moving with the speed of light. At this time Whittaker was obviously fascinated by the concept of magnetic currents and even wrote a paper (R.43) to show that Hilbert's world function could be generalized to include such hypothetical entities.

Perhaps two other papers on quantum theory are likely to be of more permanent interest—(R.54) which investigates some relations between the tensor calculus and spinor calculus with especial reference to electromagnetic theory, and (R.55) which discusses the theory which corresponds in quantum mechanics to the theory of Hamilton's principal function in classical dynamics. It will be remembered that if (taking for simplicity a problem with one degree of freedom) the co-ordinate and momentum are (q, p) at the instant t and are (Q, P) at a previous instant T , then Hamilton's principal function W is a function of q, Q and $t - T$, which satisfies the equations

$$\frac{\partial W}{\partial q} = p, \quad \frac{\partial W}{\partial Q} = -P, \quad \frac{\partial W}{\partial t} = -H, \quad (A)$$

where H is the Hamiltonian function. In the paper, a quantum-mechanical problem was considered, specified by a Hamiltonian $H(q, p)$. The variables q and p are now no longer ordinary algebraic quantities, but non-commuting operators (which may be indicated by arrows pointing from right to left).

The quantity $\bar{Q}$, which represents the co-ordinate at the instant T , is also an operator, which does not commute with $\bar{q}$ or $\bar{p}$. A function of $\bar{q}$ and $\bar{Q}$ is said to be well-ordered, when it is arranged (as it can be, by use of the commutation rule) as a sum of terms each of the form $f(\bar{q})g(\bar{Q})$. Then it was shown that there exists a well-ordered function $\bar{U}(\bar{q}, \bar{Q}, t - T)$ which satisfies formally exactly the same equations as (A), namely:

$$\frac{\partial \bar{U}}{\partial \bar{q}} = \bar{p}, \quad \frac{\partial \bar{U}}{\partial \bar{Q}} = -\bar{P}, \quad \frac{\partial \bar{U}}{\partial t} = -\bar{H}. \quad (B)$$

$\bar{U}$ may be called the quantum-mechanical Principal Function. Although equations (B) are the same as (A), the expression of the function $\bar{U}$ in terms of $\bar{q}, \bar{Q}, t - T$, is quite different from the expression of the classical function W in terms of $q, Q, t - T$; the reason being, that equations (B) are true only on the understanding that all the quantities in them are well-ordered in $\bar{q}$ and $\bar{Q}$:

but on substituting the well-ordered expression for $\overleftarrow{p}$ in the Hamiltonian $H(\overleftarrow{q}, \overleftarrow{p})$ we shall need to invert the order of the factors in many terms, by use of the commutation rules, in order to reduce H to be a well-ordered function of $\overleftarrow{q}$ and $\overleftarrow{Q}$; and this introduces new terms which do not occur in equations (4). This explains why, although the quantum-mechanical equations are formally identical with those of classical mechanics, the solutions in the two cases are altogether different.

He also introduced a third function $R(q, Q, t-T)$ which was obtained by taking the function $\overleftarrow{U}$ and replacing the operators $\overleftarrow{q}$ and $\overleftarrow{Q}$ by ordinary algebraic quantities q and Q . R was called the *Third Principal Fraction*. Writing

$$S(q, Q, t-T) = \exp iR(q, Q, t-T)/\hbar,$$

it was then shown that $S(q, Q, t-T)$ satisfies Schrödinger's differential equation for the wave-function belonging to the Hamiltonian $H(q, p)$. Many of the early derivations of Schrödinger's equation started from the principal function of classical dynamics, and after transforming a partial differential equation, which it satisfies, by writing $\psi = \exp iW/\hbar$, obtained Schrödinger's equation for ψ by omitting certain terms which were shown to be small because $\hbar$ is small. It now appeared, however, that no terms need be omitted, and that Hamilton's partial differential equation for the principal function is rigorously equivalent to Schrödinger's wave-equation, provided the principal function is understood to be the third principal function R and not Hamilton's classical principal function W .

9. *Scientific books and monographs*

Although Whittaker's original researches in mathematics and theoretical physics have had a profound effect by reason of their great range, depth and fertility, they are rivalled, if not surpassed, in interest, importance and influence by his scientific books and monographs. Three of these works call for especial mention—the treatises on analysis (B.2), on analytical dynamics (B.3), and on the calculus of observations (B.7).

A course of modern analysis, published in 1902, and in many subsequent editions with the collaboration of Professor G. N. Watson, F.R.S., was the first, and for many years almost the only book in English to provide students with an introductory account of functions of a complex variable, of the methods of analysis, and of the special functions of mathematical physics. This great work embodied many of Whittaker's own discoveries on potential functions, and it is still an invaluable work of reference. Tribute should also be

paid to Lady Whittaker who made herself sufficiently acquainted with the mathematical concepts and symbols to write out a fair copy for press.

The great *Treatise on the analytical dynamics of particle and rigid bodies*, subsequently translated into German, remains the standard work on the mathematical theory and methods. The comprehensive and magisterial character of the first part can be gauged from the titles of two chapters on 'The soluble problems of particle dynamics', and 'The soluble problems of rigid dynamics'. The second part provides an introduction to the problem of three bodies and a systematic account of the use of contact transformations. Much of this work is an exposition of Whittaker's own fundamental researches.

The calculus of observations embodied the practical experience and the theoretical work which he carried on at Edinburgh in the Mathematical Institute and as a result of his association with the actuaries in that great centre of insurance. Like the other two works just described it was a pioneer effort which opened up a new field of mathematical exploration and provided a lucid introduction to methods of practical computation.

A fourth great work stands in a class apart—the monumental treatise on *A history of the theories of Aether and electricity* (B.5) first published in one volume in 1910, and subsequently in a greatly enlarged edition of which only two volumes have been published (in 1951 and 1953). Whittaker had begun to collect materials for the third volume which would have covered the period 1926-1956, but failing health did not allow him to complete this work. The *History* provides a complete, systematic and critical account of the development of the physical theories of electromagnetism, atomic structure and of the quantum theory from their remote beginnings up to the year 1926. Of the first edition Whittaker wrote that it 'involved an immense amount of reading and historical research, which was made possible by the freedom and comparative leisure of Dunsink'. The second edition was the fruit of his years of retirement at Edinburgh and will remain as an outstanding achievement by reason of the clarity of the exposition, the comprehension of its range and the penetration of its criticism, which give it all the force and authority of an original investigation.

Two other works which were written at Dunsink are *The theory of optical instruments* (B.4) and an article for the *Encyklopädie du mathematische Wissenschaften* on perturbation theory and orbits (B.6).

10. *Historical and philosophical writings*

Whittaker's famous treatise on *A history of the theories of aether and electricity* required an enormous amount of historical research into the origins of mathematical physics, and this activity, combined with the author's intense interest in ancient, mediaeval and modern philosophy, made him an unrivalled authority on what may be called the philosophical history of physical science. He was also gifted with a peculiarly lucid and persuasive manner of exposition which enabled him to present the most abstruse and difficult conceptions in a

simple and elegant language, in which accuracy and fidelity were united with clarity and charm.

The numerous philosophical and historical papers, listed in the bibliography at the end of this notice, all bear the marks of the author's learning, literary powers and critical judgment. The twenty magnificent obituaries which he wrote of distinguished physicists and mathematicians set a new standard in this peculiarly difficult form of biography, and they provide invaluable material for the historian of physical science. Painstakingly exact and complete, eminently fair and just, urbanely critical where criticism is enlightening, and written with charming lucidity, they are also an unconscious revelation of Whittaker's scholarship, clarity, judgement and culture.

In connexion with Whittaker's writings on historical and philosophical subjects it is necessary to mention that after the death of Sir Arthur Eddington in November 1944, his sister Miss Eddington entrusted Whittaker with the editing and publication of Eddington's *Fundamental theory*, which appeared in 1946. The study of this difficult and fascinating work had a considerable influence on Whittaker and he published a number of introductory studies of Eddington's theory which did much to make it known, at least in outline, to mathematical physicists.

But Whittaker's major contributions to the philosophical history of science are to be found in his Tarner lectures at Cambridge, the Herbert Spencer lectures at Oxford, the Riddell lectures at Durham and the Donnellan lectures at Dublin.

The Tarner lectures (B.11) trace the development of theories in natural philosophy from Euclid to Eddington and provide in a modest compass a history of the great concepts and principles, especially those which have provoked lengthy and unresolved controversies. From Plato and Aristotle to Einstein and Eddington, these lectures summarize the great speculations of natural philosophers on space, time and movement, and expound in an easy and familiar manner relativistic and quantum theories together with the new 'fundamental theory' of Eddington.

The Herbert Spencer lecture (B.10) is a plea for the rehabilitation of the Cartesian ideal of philosophic certitude by means of the new discoveries in physics and the new logical apparatus developed by mathematicians.

In his Riddell Memorial Lectures (B.8) Whittaker marshalled the physical and astronomical evidence which is relevant to the enquiry whether the world had a beginning and whether it will come to an end, and emphasized with great power and religious feeling the significance of modern cosmogony for the philosopher and theologian. Without claiming syllogistic rigour for his argument he was content to exhibit the congruence of modern physical theories with the doctrines of Christian natural theology.

The Donnellan lectures (B.9) are frankly entitled 'Space and spirit, theories of the universe and arguments for the existence of God'. Whittaker had always been humorously critical of the influence of philosophy on science, and in these lectures he passed sentence on the influence of philosophy in

natural theology. The classical 'Quinque Viae' in which St Thomas Aquinas summarized the arguments for the existence of God were subjected to severe examination, on the ground that with these famous proofs 'there are entwined more or less closely, certain doctrines regarding motion, causality, cosmology and teleology, which were derived from Aristotelian sources', and which have been discredited by modern science. Whittaker undertook to restate the argument starting from the evidence for the finite age of the universe provided by contemporary cosmogony. His restatement is extraordinarily interesting to the scientist even if not entirely convincing to the theologian.*

INFLUENCE

The influence of Whittaker on mathematics and mathematicians has been remarkable both for the number of topics which he enriched by his researches and the multitude of students who have been stimulated by his teaching. It is not only his extraordinary output of original researches, authoritative monographs, historical and philosophical studies which give him such an outstanding place in the history of mathematics, but the personal influence which he exerted in the Mathematical Institute at Edinburgh, and in his famous lectures to wider audiences.

He had a peculiar facility for coining names for analytical concepts and entities, many of which have obtained such currency in the language of mathematicians that their origin and authorship is forgotten. Consider as examples, 'isometric circle', 'adelphic integral', 'cotabular functions', 'cardinal function', 'congruent hypergeometric function', 'Mathieu function', 'calamoids'. The vocabulary ranges easily over a vast field of modern mathematical physics and forms an unexpected tribute to the widespread influence of Whittaker.

G. TEMPLE

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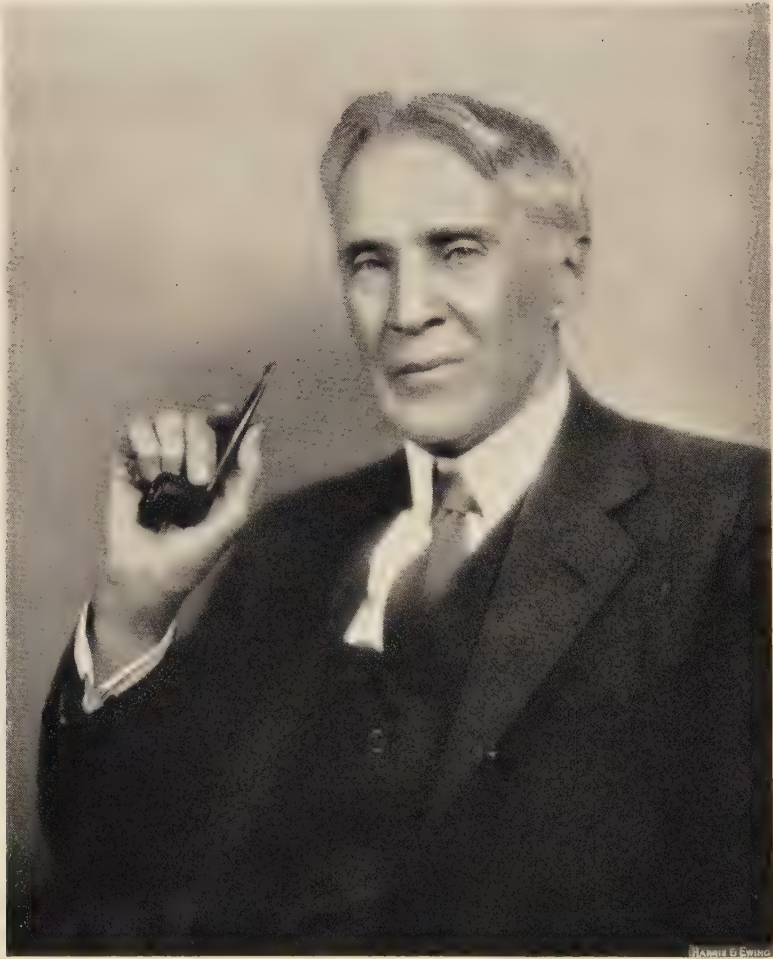
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R. W. Wood -

ROBERT WILLIAMS WOOD

1868-1955

When PROFESSOR R. W. WOOD died on 11 August 1955 he was to the younger generation of physicists a colourful legend, a representative of the past. He was, however, by no means forgotten, as a lesser man might have been at the age of 87. Many stories were still circulating about him. When I recently paid a visit to the University of Wisconsin which Wood had left in 1901, his exploits were being discussed there as if they had just happened, instead of more than half a century before. His solid scientific achievements are now a matter of record. His active mind refused to accept retirement and he visited his old room in the Physical Laboratory at The Johns Hopkins University regularly until nearly the end, even though the infirmities of old age had gradually made themselves felt. He never gave up his curiosity about things and was still actively engaged in the revision of his book on *Physical optics*. Death came to him peacefully: he passed away during his sleep without any severe illness.

Wood's active period of scientific productivity coincided with the rise of atomic physics and he made important contributions toward the increasing knowledge of the structure of the atom, chiefly through his experimental researches in physical optics. He was, however, far from one-sided and penetrated into many fields. He went wherever his insatiable curiosity led him, whether this was into different branches of physics or into all sorts of other activities such as engineering, art, crime detection, spiritualism, psychology, archæology and many others.

Wood may have inherited his interest in scientific matters from his father, Robert Williams Wood, who was born in Massachusetts in 1803, grew up in New England and was a physician in Maine until 1838. Then he went to the Hawaiian Islands where he stayed until 1866, as physician and pioneer in the sugar industry after which he returned to New England. He was active in the American Statistical Association.

From his childhood R. W. Wood, Jr., showed an absorbing interest in all sorts of phenomena in natural science and he soon started finding out for himself what made things work by trying experiments of his own. He went to Harvard University where he graduated in 1891 with a bachelor's degree in chemistry. He was apparently bored by the regularly prescribed studies but made up for the poor marks in required subjects like languages and mathematics by extra work in fields that interested him, such as geology, astronomy and psychology. After graduation he went to Johns Hopkins University for

further studies in chemistry. There he found that he was more interested in what went on in Rowland's laboratory in the Department of Physics than what he was supposed to be doing in the Chemistry Department. The following year (1892) he went to the newly founded University of Chicago, intending to get a doctor's degree in chemistry. He was not too well pleased with the routine of chemistry as it was taught there and went to Germany in 1894 for further study of chemistry. In Berlin he once more was attracted by what went on in physics, and under Rubens's influence changed permanently to a career in physics. After two years in Europe, he returned to the United States, spent a short time at the Massachusetts Institute of Technology and in 1897 accepted an instructorship at the University of Wisconsin.

During the four years he spent at Wisconsin his many-sided talents came into full blossom and he became known as one of the most promising young physicists. When Henry Rowland died in 1901, Wood was appointed his successor at Johns Hopkins University as Full Professor of Experimental Physics. Rowland was probably the most eminent physicist in the United States during the final quarter of the 19th century and to be chosen his successor came as a high honour to the 33-year-old Wood.

Wood remained at Johns Hopkins University for the rest of his life, although his stay in Baltimore was interrupted by frequent summer trips to Europe and elsewhere and by occasional sabbatical leaves. He was Professor of Experimental Physics until his official retirement in 1938 and was then appointed Research Professor, a position which he held until his death.

When one tries to describe Wood's life work and his impact on contemporary physics, one is immediately struck by the great diversity of his contributions. They range from a beginning in pure chemistry to contributions in almost every branch of physics, be it acoustics, electricity, heat or optics. He did not confine himself to the purely academic side of the subject, but often came forward with clever practical applications. One of the early examples (and a typical one) is the way he helped to relieve the hardships of the severe winters in Wisconsin. It happened regularly that the water pipes froze. Getting them thawed out was a long-drawn-out and costly procedure. Wood suggested connecting the leads of a high current transformer to the water faucets in neighbouring houses and letting the electric current produce the heat necessary for the thawing-out process. This extremely simple procedure worked so well that it established Wood's reputation as a person who could be counted on to solve practical problems, something of no small importance in a Midwestern State University, one of the chief functions for which it had been founded being to help the general population in meeting all the problems confronting a frontier society.

During his Wisconsin time Wood showed clearly the beginning of the particular genius which later made him famous with physicists all over the world. Here came the start of his studies in physical optics which were to become the main substance of his life work. The first attack on the problem is typical for Wood. The wave theory of light was, of course, well established

by the end of the 19th century. The concept of waves, wave fronts, etc., was, however, somewhat abstract. Elegant mathematical methods had been developed for demonstrating all the properties of the waves and could serve for the solution of practically any problem. These, however, meant nothing to Wood, to whom mathematical abstractions were without significance. In order to demonstrate to his students and to his own satisfaction the wave properties of light he photographed (with the Schlieren method developed by Toepler) the actual wave fronts of sound waves which have analogous properties and demonstrated vividly all the familiar phenomena which are observed when waves are reflected or refracted, etc. These photographs apparently received wide attention and he was invited by Sir Charles Boys to demonstrate them before the Royal Society in 1900.

When Wood came to Johns Hopkins University, he found a favourable atmosphere for continuing his researches in earnest. His teaching duties were light. For the next three decades a series of papers on the optical properties of simple gases appeared, which placed his name among those of the great physicists. The modern theory of the atom was then in its early stages. Scientists realized that the spectra of the elements would form the chief source of information about the detailed structure of the atoms, although the key which would make all this information accessible was not found until the fundamental discoveries by Rutherford and Bohr in 1912. Wood had started on a detailed investigation of the spectra of such simple gases as sodium, mercury and iodine. He discovered resonance radiation and studied its many puzzling features with great thoroughness and amazing experimental ingenuity. How it was polarized, how it behaved in a magnetic field or in the presence of foreign gases were things at first completely incomprehensible. Wood's experiments stimulated those of many others. During the twenties the theory had advanced far enough to be greatly aided by these experiments in its further developments. The experimental work of Wood and the increase of our knowledge about the structure of the atom were inseparable. In the late twenties after the Raman effect had been discovered, it became apparent that the most significant results would be obtained with gases. The strong background scattering seemed to make experiments with gases impossible. Wood showed that by a ridiculously simple modification the difficulties could be completely overcome.

No one who knew Wood could ever get the mistaken idea that he would confine himself to one subject. Whenever he found something that interested and puzzled him he went to his laboratory to clear the matter up. He usually could devise some simple experiment that went to the heart of the matter. Wood undoubtedly was one of the greatest experimenters of our time. Nevertheless he never worked with anything in the least complicated. His apparatus was usually made by himself, often improvised and crude looking in its external appearance but always near perfection in its essentials and capable of extraordinary performances in his own hands.

A typical example was the large spectrograph which he used during the

summer months. He did amazing things with it, using the sun as his chief light source.

Wood and his family spent the summers on an old farm on Long Island. In a barn he had improvised a laboratory and one of its features was this forty-foot grating spectrograph, probably the largest then in existence and certainly capable of giving better results than anyone had ever seen before. It was constructed from sewer pipe laid by the local stonemason. During the long months between summers when the instrument was not used, all sorts of wild life used it as a shelter and the optical path became cluttered up with spider webs. Wood's method of cleaning the tube has become a classic. He put the family cat in one end and closed this end so that the cat, in order to escape, had to run through the whole length of the tube, ridding it very effectively of all spider webs.

Wood's work was certainly not confined to the academic aspects of science. Whenever he saw a practical application he would pursue it and see to it that others knew about it. The thawing of water pipes in Wisconsin was mentioned earlier. About the same time he invented a process for colour photography and later made other contributions to photography. He was also very much concerned with the applications of the invisible ultra-violet radiation to all sorts of problems. During the first world war he developed an efficient filter, which combined with a mercury lamp, suppressed the visible light, while transmitting freely the ultra-violet. This filter is still called by many Wood's glass or Wood's filter. During the war such invisible light was used for signalling without the knowledge of the enemy. Many substances fluoresce brightly when subject to ultra-violet light and the possibility of interesting effects for stage illumination was not lost on Wood. The famous Ziegfeld, who put on spectacular stage shows in New York with a swarm of chorus girls in resplendent costumes (or sometimes without them), was Wood's neighbour on Long Island. Many of Wood's ideas on lighting tricks with ultra-violet light found their way to Ziegfeld's stage.

During the war he became acquainted with high frequency ultrasonic waves and made many experiments with them. Much of this work was done in the private laboratory of his friend, Alfred Loomis, in Tuxedo Park near New York, to which also his 40-foot spectrograph was transferred, without the sewer pipes, however.

Wood made many inventions but he was a scientist rather than inventor and did not follow them up with development work and promotion so that he rarely reaped any financial benefits from them. His work on the production of hydrogen atoms in discharges which he had undertaken in order to obtain a well-developed hydrogen spectrum led in Langmuir's hands to the hydrogen welding process. He was one of the first, if not the first, to make successful photographs in infra-red and ultra-violet light and applied them to photographing the moon and the planets in order to discover details on their surface invisible to the eye or ordinary photography. He applied this also to the detection of forgeries. He made numerous other improvements in photo-

graphic techniques, particularly as they applied to the solution of astronomical problems. He is credited with having been the first to propose the use of tear gas and with having proposed the use of air spaces around warships to dissipate the destructive power of torpedoes when the ship is attacked.

Another example shows what his combination of curiosity and experimental skill could do to solve a problem completely outside of his own field. In 1931 Wood and his wife took a trip to Egypt and at this occasion he saw in the Cairo Museum gold ornaments that had come out of Tutankhamen's tomb. This gold looked decidedly purple and the reason for this was an unsolved mystery. Some thought that the purple colour had been produced purposely by the Egyptian goldsmith by a process since lost; others believed that age had affected gold this way. No one had a clue to the mystery. Wood was allowed to take a few specimens with him and made tests on them when he returned to Baltimore. He found that the purple colour was due to a thin surface film and by spectroscopic experiments found that the gold of this film was mixed with iron and some arsenic. He produced evidence that the films were produced purposely either by mixing the necessary ingredients or by using natural gold from a locality where iron and arsenic were present as impurities. Wood was able to reproduce in his laboratory the exact appearance of the purple gold and thus solved an archaeological mystery.

When Wood became seriously interested in physical optics and began to teach this subject, first at Wisconsin and then at Johns Hopkins University, he keenly felt the lack of a book which would approach the subject as he did himself. He therefore set out to write one, and the first edition of *Physical optics* appeared in 1905. It differed from any other book in this field by the large number of experimental details which were included, details all from Wood's own experience and therefore original, fresh and full of interest. Wood admitted in the preface that he had taken over the more mathematical parts largely from other books. The experimental parts were what appealed to the reader so that a second edition became necessary in 1911, a third in 1934, and he was working on a revision of the book for the fourth edition when he died. In the preface of the third edition he stated his aim: 'I have attempted to give, in as many instances as possible, a physical picture of the processes usually described by equations.' In this he succeeded, spectacularly well in many instances. In others he perhaps overlooked the fact that not everyone had the same aversion to mathematical formulae as he had himself, and there are a few cases where his physical picture is unnecessarily elaborate, whereas a simple mathematical derivation would have conveyed the idea quite well.

I had occasion to discuss with him many parts of his book during the revision for the third and fourth editions, and the attitude he took was perhaps typical for his relations with science during his whole life. It was only natural that in a subject which had progressed as much as *Physical optics* had done, largely because of his own endeavours, there were some phases with which

he had not kept abreast but which he nevertheless wanted to include in the new edition. It would have been easy for him to ask one of his associates to write these parts for him. He never did this. He wrote the parts himself and then went to one of his friends for criticism and suggestions which he always took freely. He then rewrote the part and had it read again. This would go on until he finally felt that he had absorbed the matter and was reproducing it in his own words. Occasionally this resulted in something that others felt was more elaborate than the subject warranted but Wood refused to let others do the thinking for him.

There is an extensive biography of R. W. Wood by William Seabrook (New York: Harcourt, Brace & Co., 1941) which was written in such close collaboration with Wood that large portions of it are nearly autobiographical. This biography gives many details about his career. Much of his experimental work is conveniently assembled in his *Physical optics*. Space does not permit a full résumé here of all details, but one subject should nevertheless be singled out because through his work in this field he affected the work of many other scientists. This is his work with diffraction gratings.

Henry Rowland, in his early days at Johns Hopkins University, had made the optical diffraction grating into a precision instrument and had invented the concave grating which made possible the modern high resolution spectrograph. Rowland's engines for the rulings of such gratings were in the Physics Department of Johns Hopkins University. Wood had access to them and later was in complete charge of them. He made many studies of the gratings ruled with these engines. At the same time he made many improvements and saw to it that the machines were kept going so that gratings continued to be produced for other scientists to use. For many years these engines under Wood's care furnished practically the only source of supply for high quality diffraction gratings, without which precision spectroscopy was virtually impossible. For this reason nearly every spectroscopic laboratory in the world and most astronomical observatories had some of Wood's gratings. Only in relatively recent years have other sources of supply for such gratings become available. For, against the expectations of many, the demand for gratings has increased with the years and now they are used in industry as well as in physical and chemical laboratories and astronomical observatories.

The ruling of 15000 or 30000 or even more lines per inch, with the highest uniformity of spacing, is no small task and when there are even minute flaws in the ruling the resulting grating may be useless. The gratings produce a 'spectrum' of light and the literal meaning of the word is, of course, 'ghost'. When, however, something is called 'ghost' in plain English it does not mean the spectrum that is to be produced by the grating but flaws in the spectrum which cause spectrum lines to appear where none are supposed to be. Wood found that one class of such ghosts, the so-called Lyman ghosts, was produced by the shock to the ruling engine caused whenever the seam in the drive belt hit the driving wheel. By designing a more flexible drive he removed this difficulty entirely.

Wood designed the echelette grating: that is a grating with the grooves ruled with such a shape that they throw practically all the light into one narrow wavelength band. He also experimented with replica gratings so that one successfully ruled grating could provide many replicas with the same properties. The size of a grating is limited by the design of the engine. The astronomers in particular were looking for larger gratings to be placed in front of their large telescope objectives. Wood showed that this problem could be solved by making a composite large grating out of several smaller ones and he actually was successful in making several of these mosaic gratings. He made many other improvements in the art of ruling gratings. Perhaps more important than this is the fact that he kept the engines going and supplied a constant stream of gratings without which much of the research which contributed to the progress of physics would have been impossible.

Wood was a fascinating lecturer and a superb showman. This was already manifest during his early days in Wisconsin where he went out of his way to enliven the teaching of physics by instructive and sometimes spectacular experiments. For Wood no phenomenon became real until he had devised an experiment which clearly demonstrated it. Abstract reasoning never appealed to him; nevertheless everything he did was always carefully reasoned out. He never engaged in experimenting at random just to see what would come out but always had a carefully thought out plan. He knew all the tricks of the showman and could prepare carefully and in detail an experiment which was intended to give the impression of being improvised. He knew how to build up to a climax and it was customary that wild applause would break out even in gatherings where no one would have expected any enthusiasm. He loved stunts such as taking a mouthful of liquid air and spitting it out or breathing in hydrogen and then speaking with a changed pitch to his voice.

Wood never formed a school; he was too individualistic for that. He had, however, many students from all over the world and even now whenever two of them come together they can spend hours telling anecdotes about their former teacher. He was very fond of practical jokes. As a student he had a landlady who in his opinion showed too much interest in his affairs. One day when the streets were muddy, he took his shoes and placed footprints in his room beginning at the window, up the wall, across the ceiling and down the other wall. The reactions of the landlady are not recorded.

In the early days Johns Hopkins University was in a crowded part of Baltimore, close to sections where many negroes lived who at least at that time were noted for their superstitions. On a rainy day when there were many water puddles in the street, Wood, wrapped up in a cloak, came home past a group of loitering negroes. When he passed the group he ostentatiously spat into one of the puddles and at the same time unobtrusively threw a pellet of metallic sodium into it. The resulting yellow flames and smoke left no doubt in the onlookers' mind that they had seen Satan himself and they ran as fast as their legs could carry them.

When he gave demonstrations with ultra-violet light, often before gatherings with ladies present, he used to call attention to how brightly teeth fluoresce in ultra-violet light. Everyone would open his mouth for a demonstration until Wood remarked casually 'and you notice that all the false teeth remain black' when suddenly the mouths would snap shut again.

Wood, who worked over his own experiments, until whatever he wanted to show became crystal clear, had no patience with scientists whose results were not clear cut and even more he abhorred scientific quackery and frauds. He occasionally went to considerable lengths to set matters straight. This he sometimes did by unconventional means and often with dramatic results.

Perhaps the most famous of these cases was that of the 'N-rays', discovered by a reputable French physicist, which caused a considerable stir in 1903 and 1904 on account of the remarkable properties attributed to them. Other investigators appeared to confirm the original findings and a whole series of papers appeared in reputable scientific journals dealing with these sensational rays. In fact the French Academy awarded a gold medal and a large prize to the discoverer. Other investigators, among these Wood, could not duplicate any of the experiments. At the request of other scientists, but undoubtedly also because this appealed to him, Wood went to France to investigate the matter. The rays were supposed to be deflected by an aluminium prism. While the inventor was demonstrating the effects in a darkened room, Wood took the prism out of the apparatus and put it in his pocket without any change being observed by the inventor. Wood published this finding immediately in *Nature* and thus ended the N-rays for good.

As far as I know, no one ever suggested that fraud was involved in the affair of the N-rays. It apparently was a case of self delusion on the part of the inventor and the other scientists who claimed that they could observe them.

Wood also had ample experience with outright frauds and fakers. Some claimed to have secret inventions for which they sought financial backing, others claimed a cure for strange ailments, others again could communicate with the spirits of the departed. Wood had no patience with any of them and used much ingenuity to trap them and had spectacular success in many cases. Mediums soon became wary and after such experiences did not want to have anything to do with Wood.

The following example has all the imprints of Wood's method though I cannot vouch that it originated with him. During the last war there was a self-styled inventor who claimed to have discovered a powerful explosive which in his opinion would supersede all hitherto used explosives while the military experts considered it worthless. The inventor had, however, powerful friends among the politicians in Washington and therefore the Aberdeen Proving Ground, where Wood was a consultant, could not refuse to give the inventor the opportunity for a demonstration. It was agreed that the effect of the explosive should be tested with goats to be tethered at various distances from the explosion. The day of the test arrived with senators and generals

present and the newspapers full of the importance of the occasion. The explosion went off but the goats, even the nearest ones, calmly continued to graze as if nothing had happened. Nothing more was ever heard of this particular explosive.

What had not been made public was, that on Wood's advice the goats had been made to graze for weeks in the immediate vicinity of the big guns that were tested at Aberdeen so that noise did not startle them at all. While undoubtedly the worthlessness of the explosive would eventually have been demonstrated by the more usual methods, Wood's way presented a short cut which saved a great deal of time, which the explosives experts could then use for better purposes.

Wood could attack, with the same zest that he bestowed on the most important problems in physics, other questions that caught his fancy whether they were on the fringes of science or had nothing to do with it.

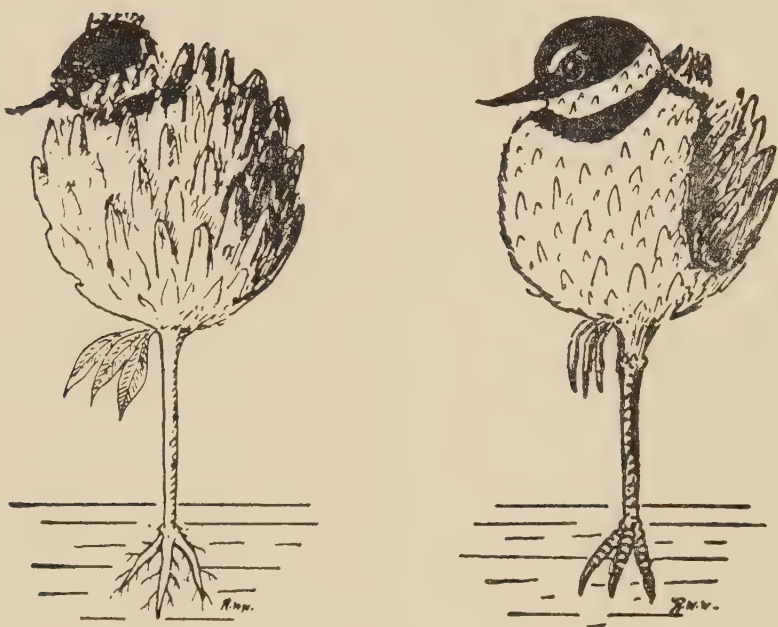
He started as a boy and kept it up until his death. As a student at Harvard he became interested in hallucinations and in order to gain first-hand experience secured a quantity of hashish, swallowed it and recorded in minute detail the visions and hallucinations he experienced, and reported these as a thesis in a course of psychology he was then taking. He also sent the account to a New York newspaper and was disappointed when it was published without sensational headlines.

In the matter of publication, Wood was never a publicity seeker but was so interested in whatever he was doing that he felt others should be quite as interested to read about it. He often sent his scientific papers simultaneously to journals in the United States, Britain and the Continent and saw to it that the daily press was also informed. When he was very impatient he sent communications to *Nature* by cable.

To come back to Wood's extrascientific activities: they were too numerous for all to be mentioned here, because he was interested at one time or other in practically everything. He was a skilled boomerang thrower and introduced the Hawaiian surfboard to the Long Island beaches. He travelled before 1900 through the Wisconsin countryside in one of the earliest automobiles seen in that part of the country. He was a gifted painter in water colours. For a time he tried his hand at science fiction in collaboration with Arthur Train, an author of popular stories, but after getting a few stories published he gave up this sideline.

He was on the other hand very successful with a series of poems, first published in 1907, and probably many know him as the author of *How to tell the birds from the flowers* who never heard of Wood, the physicist, for this volume went through about twenty editions. He had written these nonsense verses to amuse his children and illustrated them by clever drawings. An example is reproduced here with permission of the publishers (Figure 1).

It is not surprising that Wood with all his scientific curiosity should be fascinated by crime detection and he lent a hand in the solution of a number of criminal cases in New York and Baltimore.



The Clover. The Plover.

The Plover and the Clover can be told
 apart with ease,
 By paying close attention to the
 habits of the Bees,
 For ento-molo-gists aver, the Bee
 can be in Clover,
 While ety-molo-gists concur, there
 is no B in Plover.

—2—

FIGURE 1. Illustration reproduced from Wood's book *How to tell the birds from the flowers*.

The scientific curiosity which he showed in all matters manifested itself even when, a few years before his death, he suffered from a heart attack. As these things go it was not a very severe attack but he must have driven his doctor to distraction by the intense interest he showed in his own symptoms. It was a great disappointment to him when he realized that he was not the first one to go through this experience.

Wood always had close ties with British scientists. These date from 1900 when he was invited to lecture on his process of colour photography before the Royal Society of Arts and received at the same time an invitation for a demonstration of his photographs of sound waves before the Royal Society. In 1904, in connexion with the Cambridge meeting of the British Association for the Advancement of Science, he was the guest of Lord Rayleigh at his country house and private laboratory and there was from then on a frequent exchange of ideas between the two great scientists. In 1919 he was elected Foreign Member of the Royal Society. He received his first honorary degree from the University of Birmingham and one of the latest ones from Oxford University, with one from the University of Edinburgh in between. He was an honorary member of the Royal Institution and of the London Optical Society and an honorary fellow of the London Physical Society. He received in 1899 a medal of the Royal Society of Arts for his invention of the diffraction colour process in photography and in 1938 the Rumford Gold Medal of the Royal Society for his achievements in physical optics, a distinction which he valued perhaps most of all. The *Philosophical Magazine* and later the *Proceedings of the Royal Society* were his favourite journals for his own publications.

Britain, thus perhaps more than any other country, showed appreciation for Wood's genius, although he did not lack distinctions in his own country and abroad. He was a member of the National Academy of Sciences in Washington, of the Academia dei Lincei in Rome, of the Russian Academy of Science in Leningrad, the Royal Swedish Academy in Stockholm and the Indian Association for Science in Calcutta, among many others. The University of Berlin awarded him an honorary doctor's degree in 1934 and Johns Hopkins University honoured him in the same way when in 1951 he had finished his fiftieth year as Professor at that Institution.

During that same year his closest associates in Baltimore honoured him at an intimate dinner to which the wider circle of his friends had been invited to send messages. The messages poured in from all corners of the world. They showed how much Wood and his achievements were still in the minds of physicists.

From the foregoing, the reader will have gathered that Wood was by no means a one-sided scientist but that he had wide interests and made contributions to many diverse fields.

In his private life he was far from being a recluse. In 1892 he married Gertrude Ames, who also came from a New England family and who was his constant companion for more than 60 years, although she herself had no interest in scientific things. The Woods led a very active social life in Balti-

more, at their summer place near Easthampton on Long Island, and during their travels abroad. They had a very wide and interesting circle of friends. Mrs Wood provided the family stability without which a man of Wood's temperament might have found life occasionally very difficult. He is survived by Mrs Wood, their three children and many grandchildren.

His biographer, Seabrook, called him 'a small boy who never grew up' and there is probably a good deal of truth in this for notwithstanding much sophistication his fundamental approach to everything was that of a small boy fascinated by something new which he wants to take apart to see what makes it work.

The secret of Wood's greatness is probably that he could recognize a problem that could be dealt with by experimental methods, that he could then reduce the experimental technique to something very simple and that in carrying out the experiment he could distinguish the essential points from the confusing details. Physicists will look back with nostalgia to this era where one person working practically by himself could make major contributions to the frontier fields of his science and they will think of R. W. Wood as the protagonist of this era.

Perhaps techniques in physics have inevitably changed as so many other things have changed since the two wars. Now we have become accustomed to a state of affairs where even a moderate effort to get beyond what is already known often involves the expenditure of millions of dollars and requires the co-operation of many persons, scientists, technicians and engineers and an organization that will make such co-operation effective. Many will think back to the times of R. W. Wood as the good old days. Perhaps there will again be persons who can make this era revive.

G. H. DIEKE

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